PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

April 1889.

XIV. *On the Dimensions of Electromagnetic Units.* *By* Prof. G. F. Fitzgerald, *F.R.S.**

Some attention has lately been called to the question of the dimensions of electromagnetic units, but the following obvious suggestion seems to have escaped notice.

The electrostatic system of units may be described as one in which electric inductive capacity is *assumed* to have zero dimensions and the electromagnetic system as one in which the magnetic inductive capacity is assumed to have zero dimensions. Now if we take a system in which the dimensions of both these quantities are the same, and of the dimensions of a slowness, *i. e.* the inverse of a velocity, $\left[\frac{T}{L}\right]$, the two systems become identical as regards dimensions, and differ only by a numerical coefficient just as centimetres and kilometres do. There seems a naturalness in this result that justifies the assumption that these inductive capacities are really of the nature of a slowness. It seems possible that they are related to the reciprocal of the square root of the mean energy of turbulence of the æther.

* Read February 23, 1889.

XV. *Note on the Measurement of Resistance.*

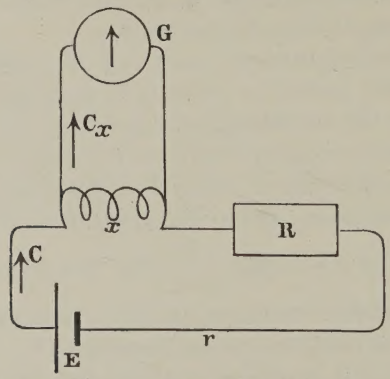
By Dr. J. W. W. Waghorn*.

It is well known that an unknown resistance x can be determined in terms of a known resistance R, if both are arranged in series with a cell, and if the deflexions of an electrometer be observed when connected in turn to x and to R.

If C is the current in the circuit, the differences of potential at the terminals of x and of R are respectively Cx and CR.

It is also well known that any voltmeter may be substituted instead of the electrometer, if the resistance of the voltmeter-circuit is very large compared to x and R.

But it may not have been generally observed that the same method may be employed whatever be the relative resistances of the voltmeter and the conductors; and is true even if the usual conditions are reversed, so that the voltmeter-resistance is very small compared to x and R. Any galvanometer may take the place of the voltmeter.



Let G be the resistance of the galvanometer;
E be the E.M.F. of the battery;
r be the resistance of the battery and connecting-wires.
Let C be the strength of the current on the undivided part of the circuit;
C_x be the strength of the current through the galvanometer when its terminals are connected to x;
C_R be the strength of the current through the galvanometer when its terminals are connected to R.

* Read February 23, 1889.

Then

$$C = \frac{E}{R + r + \frac{Gx}{G+x}},$$

and

$$C_x = C \times \frac{x}{G+x} = \frac{E}{R + r + \frac{Gx}{G+x}} \times \frac{x}{G+x}$$

$$= \frac{Ex}{RG + Gx + rG + rx}.$$

Also, substituting R for x,

$$C_R = \frac{ER}{RG + Gx + rG + rR}.$$

If x and R are large compared to r, the denominators of the above fractions will be practically identical; and the approximation will be still closer if, as would usually occur in practice, x is not very different from R.

The sensitiveness of the method is practically the same as that obtained in the ordinary "substitution" process of determining, but has the small advantage over the latter of not requiring a knowledge of the resistance of the galvanometer.

XVI. *On the Use of* Lissajous' *Figures to determine a Rate of Rotation, and of a Morse Receiver to Measure the Periodic Time of a Reed or Tuning-fork. By* Prof. J. Viriamu Jones, *M.A.**

It is sometimes of importance to determine with great accuracy the angular velocity of a rotating body at a given instant. For instance, in measuring an electrical resistance in absolute measure by the British Association method or the method of Lorenz, the rate of rotation of the revolving coil or disk must be known with full accuracy at the time when the reading of a galvanometer-needle is taken.

The method I have to bring before the Physical Society in this note consists in obtaining, by means of Lissajous' figures, equality of period between the rotating body and a reed maintained in vibration electrically, and then subsequently determining the vibration-period of the reed.

* Read March 23, 1889.

(i.) *The use of* Lissajous' *Figures to obtain Equality of Period between the Revolving Apparatus and the Reed.*

In one end of the axle of the revolving apparatus (which in the application of this method in the laboratory of the University College, Cardiff, is a Lorenz disk rotating about a horizontal axis) a pin is placed excentrically. The pin fits accurately in a hole in a rod free at one end, and constrained to move only longitudinally at the other.

Fig. 1 represents an end view of the axle M M M. The excentric pin is indicated by the dotted circle at B. The pin fits into a hole in the rod S S free at the end C, the other end, A, being constrained to move only longitudinally.

When the apparatus is in rotation the excentric pin impresses on the rod a vibratory motion; and the vibration-period of the rod is equal to the revolution-period of the revolving disk.

To the free end of the rod a mirror, T, is attached.

In the apparatus in my laboratory, already mentioned, the axle is horizontal and the rod moves in a vertical plane.

The reed is a steel bar 100 centim. long, and its section is rectangular, 1·51 centim. by ·60 centim. It is clamped in a massive iron frame, and by moving the bar in the frame the vibrating segment may be made longer or shorter so as to vary its vibration-period. The period may be further adjusted by the movement of a rider.

Fig. 2 is a plan of the reed and its electrical arrangements, which are a little different to those in ordinary use in order to allow, without inconvenient readjustment of the springs, the lengthening or shortening of the vibrating segment.

A A A is the steel bar.

C C the iron frame to which it is clamped by the bolts and nuts D, D. This stand is clamped to a large wooden block resting on a concrete floor.

F F F is a wooden piece attached at N N to the iron frame, and bearing the electromagnet M.

On the opposite side of the vibrating bar to the electromagnet there is a pair of springs S_1. One of the pair is always in contact with the bar as it vibrates, and the other, fitted with screw-adjustment, makes and breaks contact with the first. The first spring is connected through the electro-

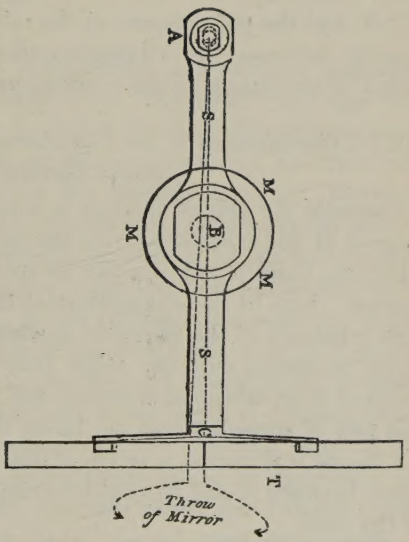


Fig. 1.—Vibrating Mirror on Disk. Full size.

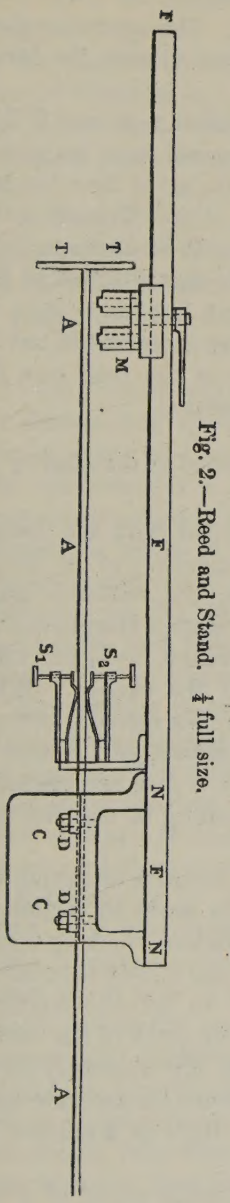


Fig. 2.—Reed and Stand. ¼ full size.

magnet with the pole of a suitable battery, the other pole being connected with the second spring. The apparatus gives no trouble, and once started the reed will vibrate for hours without attention.

To the free end of the vibrating segment a mirror T T is attached. In my apparatus the reed moves in a horizontal plane, and so its plane is at right angles to that of the vibrating rod attached to the axle of the rotating disk. Therefore the image of a spot of light seen by reflexion in both mirrors (viz. that attached to the rod on the axle, and that attached to the reed) will for synchronism be drawn out into some form of ellipse, and the permanence of the ellipse is an exact test of equality between the vibration-period of the reed and the period of revolution of the rotating-apparatus.

(ii.) *Determination of the Vibration-period of the Reed by a Morse Receiver.*

Opposite the pair of springs S_1 connected with the electromagnet M and concerned with the maintenance of the reed in vibration, there is another pair of springs S_2, similar in every respect. One of these is connected through a Morse receiver with a battery of the requisite electromotive force, the other being connected with the other pole of the battery. When the reed is in vibration it makes and breaks contact between this pair of springs; and we have, therefore, on the tape of the Morse instrument a series of dashes; a dash and the blank space between it and the next corresponding to one vibration of the reed.

On the same tape, side by side with the track of the inking-wheel of the Morse instrument, a pen is made to record the movement of the pendulum of the standard clock in a sufficiently ordinary manner. We have then merely to count the number of dashes in a given time, and we have the number of vibrations in that interval, and hence the pitch of the reed.

The electrical connexion is made in my laboratory by the escapement-wheel of the clock; and hence for accurate result the time taken must be an integral multiple of a minute, the period of revolution of the wheel.

This method of determining the vibration-period of a reed or tuning-fork has seemed to me to be of extreme simplicity, and by taking a sufficient interval of time may be made of

any desired accuracy. A Morse receiver will spin out its tape at a speed of 40 feet a minute, and the ordinary rolls of tape will last for at least 15 minutes when it is doing so.

The limit of accuracy in this method of determining the rate of vibration is, I find at present, the constancy of vibration of the reed. It has been a surprise to me to find that the vibration-period is not constant to much more than 1 part in 1000.

In conclusion, I desire to express my thanks to Mr. John Gavey, of the S. Wales Telegraphic Department, for the help he has rendered me in connexion with this matter.

University College, Cardiff,
March 22nd, 1889.

Prof. Ayrton, whilst recognizing the extreme importance of determining speed accurately, suggested that the inconstancy of the reed may be due to the impulse being given at the end instead of at the middle of its swing; and recalled an experiment, shown before the Society by Prof. Perry and himself, in which the pitch of an electrically-driven fork was varied greatly by altering the adjustment of the contact-screw Referring to Dr. Thompson's modification where two tuning-forks drive each other, it was pointed out that the method requires the synchronism of the two forks to be very exact.

Mr. Blaikley inquired whether any doors were opened or closed during the experiments, as the pitch of a reed is affected by the size of its resonance-chamber. He also stated that the pitch of reeds driven pneumatically could be maintained constant to 1 part in 10,000, and mentioned that two forks nearly in unison influence each other's period when near together.

Referring to the two forks mentioned by Prof. Ayrton, Prof. S. P. Thompson said it was advantageous to mount such forks on sounding-boxes, for when placed at a suitable distance apart they then exert considerable mutual control. The sketch put on the board by Prof. Jones led Dr. Thompson to suppose that a perfect method of driving-forks had been devised; for two springs were shown touching opposite sides of the bar, and such an arrangement might be used to complete the circuit, only when the reed is in the middle position. He also believed that forks give greater constancy than single reeds, and mentioned some recent improvements in which one

prong of an electrically-driven fork is made of phosphor-bronze.

Mr. T. H. Bakesley, reasoning from ideas suggested by Mr. Stroh's experiments on vibrating membranes, concluded that the periods of forks placed at $\frac{1}{4}$ wave-length apart would not influence each other.

In reply to a question from Mr. F. J. Smith, Prof. Jones thought there could be no "creeping" of the reed through the clamp. He also stated that he had been led to use a reed from the results obtained in Delaney's system of telegraphy, and the fact that Lord Rayleigh considered electrically-driven forks satisfactory.

XVII. *On Physico-Geometrical Models.*

By Prof. A. S. Herschel, *D.C.L., F.R.S.**

(Abstract.)

The subject had suggested itself to the Author as an important one for study some five years ago, from the possibility which he then discovered of effecting a four-limbed mechanical cycle, similar to a thermodynamic cycle, with a conically revolving pendulum, the mathematical conditions of whose motion involved the logarithm of the circle radius of the knob's revolution, as a quantity equivalent to entropy in the thermal cycle. The mechanical cycle may be effected as follows:— Suppose the pendulum-knob to be revolving at velocity V_1 and radius r_1; imagine the controlling force to decrease so that the radius varies from r_1 to r_2, the velocity remaining constant at V_1; then increase the controlling force to its original value, and simultaneously impart velocity to the knob to keep the radius constant at r_2, the final velocity being V_2. Next, further increase the controlling force so as to cause the radius to decrease to r_1 at velocity V_2, and, finally, decrease the controlling force to its initial value, simultaneously retarding the knob till the velocity is V_1 at radius r_1. The nett energy concerned in the operations was given as

$$W=(V_1^2-V_2^2)\phi,$$

where $\phi=\log\frac{r_1}{r_2}$; and the cycle affords an illustration of trans-

* Read February 9, 1889.

formation of gyratory motion into energy of reverberatory push along the axis. The logarithm ϕ is also measured by the area of a hyperbolic sector, and is connected (as was shown) with a sector of the circle which a lamp's conical beam would inscribe upon a square glass screen or plate, dividing a long room of square cross-section into two parts. The conical beam prolonged through the glass then traces on the walls beyond hyperbolas, whose sectors from their summits are related to the circle's corresponding sectors by a well-known hyperbolocyclic trigonometrical connexion. From this mode of constructing the connexion, and from a discordance which it shows at the asymptote extremities with Euclid's definition of parallel straight lines, the Author concludes that cubic space cannot be continuous in its structure, but must be, in a physically constructive sense, like material particles, indestructively atomic or molecular. The models showed modes of constructing cubic space in various ways, chiefly by means of tetrahedra, octahedra, and dodecahedra; the common element of form in these being also shown to be the right-angled tetrahedron or "biquoin," obtained by dividing a cube into six equal parts by three planes through its diagonals. A model of Sir W. Thomson's soap-form figure, and some wooden models representing Haüy's polyhedral atoms and their combinations, were used to illustrate the structural view thus taken of geometry; and chemical-figure models of the ring of tetrahedral carbon molecules in benzene, and of the asymmetric groups of similar atoms in active and inactive tartaric acid were shown.

XVIII. *The Rotation of the Plane of Polarization of Light by the Discharge of a Leyden Jar. By* Dr. Oliver Lodge*.

The current produced by the discharge of a Leyden jar is so violent while it lasts, that those phenomena which depend upon the value of a current independently of its duration are well excited by it. Such are the induction of currents, the production of magnetism, and the rotation of the plane of polarization.

Nothing is easier than to wind a quantity of thin gutta-

* Read March 9, 1889.

percha-covered wire round a piece of heavy glass, and to witness the bright flashing of a dark field between polarizer and analyser whenever a large Leyden jar is sparked through the coil, the source of light being a paraffin-lamp or gas-flame. The suddenness of the effect suggests, of course erroneously, that it is an illumination caused by the light of the spark which one is looking at.

The fact that the discharge is oscillatory, and that the restoration of light in the dark field is oscillatory too, is proved by the fact that an adjustment of the analyser to one side or the other of complete darkness has just the same effect on the result. It is proved also by the fact that a biquartz exhibits no change in its sensitive tint during the discharge; similarly also Jellett, and other such reversible detectors, would be useless for the purpose: the effect is an irreversible one. Nevertheless some sort of measure of the effect can be made by finding the position of the analyser whereat the brightness of the field suffers no appreciable change at the occurrence of the spark, because in one position the oscillation on one side will darken it just as much as that on the other side brightens it.

It seemed to me possible that if the effect could be rendered pretty considerable a slight *darkening* of the field might be obtained with some adjustments, because some arcs of the sine curve have an average ordinate less than their middle ordinate. But when a fairly bright field winks slightly it is not easy to say whether it winks brighter or darker, and after all I do not know that it much matters.

The main interest in the experiment seemed to me to lie in the evidence it afforded of practical instantaneity in the development of the property in the substance under examination; and in order to try oscillations of much greater frequency than those I first used, I got my assistant, Mr. Robinson, to make a long tube of carbon disulphide with which to repeat the experiment in a sensitive manner.

Now, a most interesting experiment of Villari*, in which he whirled a drum of heavy glass up to 200 revolutions per second between the poles of a magnet, and perceived the

* Pogg. *Ann.* cxlix. 1873, p. 324. Translated from the *Rendiconti Istit. Lombardo*, ser. 2, vol. iii.

electro-optic effect to diminish from 100 revolutions per second upwards, and ultimately nearly cease at high speeds (say 180 per second), had led one to suppose that some distinct time was necessary for the production of the effect—something between $\frac{1}{800}$ and $\frac{1}{400}$ second. But on referring to that most useful summary of Electrical Science, Prof. Chrystal's article in the Encycl. Brit., I found, along with a quotation of Villari's experiment, a statement that Professors Bichat and Blondlot *, of Nancy, had by means of Leyden-jar discharge proved that practically no time was necessary. I accordingly procured a copy of the volume of the *Comptes Rendus* from London, and there found an all too brief account of a most beautiful series of experiments, by which they considered it proved that if any time is required, it is less than the $\frac{1}{30,000}$ second. The skill of these French Professors in optics, and their previous researches in connexion with the Faraday effect in various substances, are well known; and what I have to show to-day is practically nothing more than a repetition of their experiments with the theory worked out.

I take either a bit of heavy glass in its helix, or, for projection, a tube of CS_2 a yard long surrounded by four large helices, each containing about 80 yards of gutta-percha-coyered No. 16 wire; and on passing the discharge from a battery of several jars the field flashes out bright, in what may be (if one is looking direct towards the hot lime) quite a dazzling manner.

I have just received a post-card from M. Bichat, in answer to an inquiry, saying that the coil they used was the secondary bobbin of a Rühmkorff coil with a resistance of 5000 ohms. Now, whether this was by accident or by design, it was difficult for them to use a coil more suited to the purpose, or one that would give a larger effect, as I shall show directly. The Leyden jars employed by them were either one or two, about 18 inches high and 6 inches in diameter. We shall find that the effect increases in direct proportion to the capacity of the jars.

To find out whether any time was required for the development of the effect, they made the light coming through the tube illuminate a slit, the spark being made to illuminate

* *Comptes Rendus*, xciv. 1882, p. 1590.

another slit close above the first, and then both slits were examined in a rotating mirror. Both were spread out into a discontinuous band, and the serrations of the one agreed as nearly as could be seen with the serrations of the other. Thus proving in a beautiful manner that there was practically no lag of the effect behind its cause, and thereby contradicting the conclusion of Villari *.

Meanwhile I had been doing similar experiments, but with a bobbin of much smaller inductance, and using a still smaller capacity, my object being to find the greatest frequency able to show the effect distinctly. If, for instance, heavy glass or CS_2 was able to follow oscillations of some million per second, there could be no further question but that Villari's conclusion was wrong. I find that the CS_2 is able to show the effect when the rate of alternation is 70,000 per second ; and though the short length of heavy glass available does not enable me at present to make quite the same statement for it, I have no reason to suppose it in any way inferior. In fact, experiment distinctly suggests that the effect is practically instantaneous ; but as to the degree of instantaneity I shall be able to make a more exact numerical statement later on.

It may be of interest to the Physical Society to have the oscillatory character of the restored light demonstrated, and there is no difficulty in the experiment. One sets the analyser to as near darkness as possible, one receives the trace of residual light upon a rotating mirror, by which it is spread out into a faint band ; and on then sending sparks through the coil of wire round the tube of CS_2, the band brightens and presents a distinctly beaded appearance at every spark.

Rotating the analyser a little, every alternate bead grows fainter, while the other alternate ones brighten, thus proving most directly the oscillatory character of the light and of the Leyden-jar discharge.

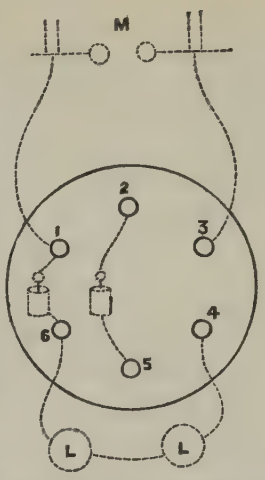
* They also mention an unpublished experiment by MM. Curie and Ledeboer, in which a disk of glass was spun between the poles after the manner of a Foucault disk of copper, so that the path of the light was parallel to the axis of rotation, instead of perpendicular to it as in Villari's drum arrangement, and in that case no diminution was observed. If this experiment is yet published I am ignorant of it.

Still more feasible is it to spread out the light of the spark itself into a serrated band, but in making this visible to an audience it is well to save time by exciting the jars with a large induction-coil instead of a Wimshurst machine, because not only can sparks be thus got much more frequently, but each spark is multiple—the jars filling and overflowing several times during the one coil-discharge. The multiple or intermittent spark is analysed by the revolving mirror into a number of serrated bands one after the other, and while one band is in the field of view of one part of the audience another band may be visible to another.

Although when a suitable circuit is employed the analysis of the spark in a mirror such as is used for manometric flames rotating not more than three or four times a second is easy, yet with ordinary discharging circuits I have used small mirrors spinning 200 times a second and failed to see any certain trace of oscillation ; while, as is well known, Wheatstone used mirrors rotating 800 times a second, and got the image-spark only barely elongated : not in the least serrated. It may be worth while, therefore, to state the kind of circuit which I have recently employed. The capacity consists of a couple of condensers built up in the laboratory with double thicknesses of window-glass alternating with tinfoil, and the whole flooded in paraffin till it is a solid mass in a box of teak weighing a couple of hundredweight. These condensers stand a considerable length of spark, 3 or 4 inches for instance, but their strength is not in the least called out in the experiments now related. Their capacity when joined up parallel is ·048 microfarad. These are often supplemented by a battery of ordinary Leyden jars and by single jars, which altogether raise the capacity to ·074 microfarad or 670 metres. To vary the capacity in the ratio 1, 2, 4, I use a switch consisting of 6 glass pillars, each with a bit of close-fitting glass tube cemented on the top to serve as a mercury cup. The leading wires are held in the mercury cups by indiarubber umbrella-rings round the glass pillars ; and insulated wire bridges easily make connexion between one pillar and the next.

In the annexed figure dotted lines indicate sufficiently the

permanent connexions. M being machine and discharger, L being circuit and coils. The movable connexions are made in pairs as follows :—



For series or cascade, connect 2, 3; 4, 5.

For one condenser only, connect 3, 4.

For both condensers in parallel connect 1, 2 ; 3, 4 ; 5, 6.

As regards circuit, one of the coils I use is a hank of 440 yards of thickly covered No. 16 G. P. wire as it came from the maker, with a self-induction of ·048 secohm and a resistance of 3·75 ohms.

The coils on the CS_2 tube consist of the same kind of wire ; they have a resistance rather greater than the above, and a combined self-induction of about ·008 secohm.

A number of gigantic electric-light cables have been inserted in the circuit at different times, one on a bobbin like two cart-wheels being kindly lent by the Engineers of the Liverpool Electric Supply Company ; but the easiest way of getting very large self-induction is to use No. 20 or No. 22 G. P. or I. R. wire not too thickly covered. I have recently obtained a large coil wound so as to give maximum self-induction which I estimate as 7 or 8 secohms ; I have not yet used this, but I frequently use a number of coils of No. 21 wire packed together as close as is easily possible, and making a total self-induction of 1 secohm or ten thousand kilometres.

Without going into further details, I may say that the observed frequency of the oscillations, as estimated from the appearance of the serrations in the revolving mirror, or by the pitch of the musical note accompanying the spark, agrees very respectably with the frequency as calculated from

$$\frac{3\times10^{10}}{2\pi\sqrt{\left(\frac{L}{\mu}\cdot\frac{S}{K}\right)}}.$$

Returning now to the magneto-optic effect of the Leyden-jar discharge, I make out its theory to be as follows :—

The current at any instant during the discharge is

$$C = \frac{V_0}{pL} e^{-mt} \sin pt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $m = \frac{R}{2L}$ and where $m^2 + p^2 = \frac{1}{SL}$.

The difference of magnetic potential between the two ends of a rod of length l surrounded by a very long solenoid of n_1 turns of wire on unit length of it is

$$4\pi\mu C n_1 l.$$

The Verdet constant for carbon disulphide, or the rotation of the plane of polarization per unit difference of magnetic potential is, according to the determination of Lord Rayleigh, ·04202 minute for a temperature of 18° Centigrade, and for sodium light. Calling this k, we have the rotation effected by the current

$$\theta = 4\pi k n_1 l \mu C. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the case of the heavy glass experiment a helix longer than the stalk of glass is employed ; but in the case of the long tube of CS_2 and four helices there will be some correction necessary for the ends of the helices. This correction is, however, well understood, and there is no special need to introduce it at present.

Now the total amplitude of the light falling upon the analyser being a (this amplitude being all stopped if the analyser is set to darkness, and all transmitted if it be turned 90°), the component amplitude which will at any instant get through the analyser set to darkness, when the plane of polarization has been rotated through an angle θ, is

$$a \sin \theta ;$$

and the amount of light which gets through during the time τ during which an impression is capable of being accumulated on the retina, *i. e.* before it has begun to die away as fast as it is produced, is

$$\int_0^\tau (a \sin \theta)^2 dt.$$

The amount of light which would get through in the same time if the analyser were set to maximum brightness would be

$$a^2\tau.$$

These expressions, then, furnish the measure of the respective effects upon the retina; and so we have

$$\frac{\text{apparent brightness restored by the spark}}{\text{maximum brightness possible}} = \frac{\int_0^\tau a^2 \sin^2\theta\, dt}{a^2\tau}.$$

Now inasmuch as τ is comparable to the time of persistence of retinal impression, being perhaps equal to it, and since this time is greatly longer than any ordinary duration of a spark-discharge, the above ratio of the relative brightnesses reduces, for practical purposes, to

$$\text{B} = \text{relative brightness} = \frac{1}{\tau}\int_0^\infty \sin^2\theta\, dt, \quad . \quad . \quad (3)$$

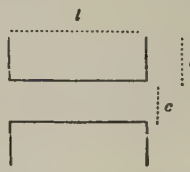
and this is what the eye will observe.

Referring back to equations (1) and (2) we have the means of determining this quantity. I do not know how to do it completely, but for the case when θ is moderately small the integral is easy, viz.:—

$$\text{B} = 16\pi^2k^2\mu^2n^2\frac{\text{V}_0^2}{p^2\text{L}^2\tau}\int_0^\infty e^{-2mt}\sin^2 pt\, dt$$

$$= 160\, k^2\mu^2n^2\,\frac{\frac{1}{2}\text{SV}_0^2}{\text{R}\tau}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The effect thus depends directly on the square of the total number of turns of wire employed, directly on the energy of the static charge used, and inversely on the resistance of the circuit.



To find the best size of wire to wind on a bobbin of given size, for the purpose, one can write down the value of n^2/R; given the length of the bobbin as l, its depth of winding-space b, the diameter of its empty core c. Call the radius of the uncoveredwire used ρ, and its radius when covered ρ'.

First, supposing no appreciable resistance in the rest of the circuit, it comes out

$$\frac{n^2}{\text{R}} = \frac{lb}{4(c+b)}\left(\frac{\rho}{\rho'}\right)^2, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which means that the size of the wire does not matter, but that it is important to keep the covering thin. Only then unfortunately the discharge is liable to burst the covering. In my helices $\rho' = 3\rho$, which is rather excessive, because it reduces the visible effect to one ninth of what it might be if no covering were used.

It is impossible to press this to its natural consequence of using only one turn of thick sheet copper, because the resistance of the rest of the circuit has been neglected. Taking it into account, as r, the relation becomes

$$\frac{n^2}{R} = \frac{\dfrac{lb}{4(c+b)} \cdot \left(\dfrac{\rho}{\rho'}\right)^2}{1 + \dfrac{4r}{lb(c+b)}(\rho\rho')^2}, \quad \ldots \quad (6)$$

which contains the product of the sizes of covered and uncovered wire as well as the ratio ; and this product occurs in the term containing r, the external resistance. Hence, to keep this term small, it is desirable to use wire thin enough to throw the major part of the resistance into the bobbin ; and there is no limit to the thinness of the wire that may be advantageously employed, until the thickness of the covering bears too high a ratio to the whole. And inasmuch as insulation thickness may be more judiciously distributed between layers than between consecutive turns, it is obvious how extremely suitable a coil for the purpose is the secondary of a Rühmkorff.

I said that I did not know how to evaluate the complete integral involved in (3) when θ is not small ; but, as usual, I sent the problem to my brother, and he speedily reduced it to a form equivalent to this :—

$$B = \frac{L}{R\tau} \int_0^A \frac{1 - J_0(x)}{x} \cdot dx, \quad \ldots \quad (7)$$

where

$$A = 8\pi k n \mu V_0 \sqrt{\left(\frac{S}{L}\right)}.$$

I have asked him to write a short appendix to this paper.

Returning now to consider the meaning of these equations, and attempting a numerical estimate of what to expect in practice, we shall find that though the instantaneous rotation

expressed by (2) is enormous, being quite possibly 60 or even 180 degrees, yet the restoration of light expressed by (4) is but feeble, and only some ten thousandth part or so of what could be gained by rotating the Nicol. This explains why it is fairly easy to analyse the restored light into a beaded band, because then one gets the effect of the separate oscillations ; and they are very bright though very momentary.

Looking at the formula (4), it is clear that to get large effects it is desirable to use a large battery of jars, and to charge to a high potential, only that one is afraid of breaking down the coil. Large capacity and a great number of turns of wire are the safest ways of increasing the effects.

One sees also that extra self-induction in the circuit does neither good nor harm to the resultant effect. It diminishes the effect of each oscillation, but it prolongs the time during which they last. And it is the total "*action*" of the series of decaying swings which the eye perceives. One sees that extra resistance in the circuit is wholly bad.

As to the cause of what must now be regarded provisionally as the erroneous conclusion of Villari I see no reason to doubt his experiments, though he does not give sufficient details to enable one to arrive at a perfectly satisfactory judgment on all points ; it is practically certain that he did get a much diminished effect on spinning the flint-glass drum between the poles of the magnet and sending the light along successive diameters of the drum.

But the cause of this I venture to suggest is possibly to be found in the state of strain into which the glass will be thrown by centrifugal force. It may be said that, if so, the light ought to have been similarly affected even when no magnetic field was employed ; and Villari expressly says that this was not the case.

But then it is to be noticed that, when no magnetic rotation is attempted, the aspect of the plane of polarization to the stress remains constant throughout the journey; and if light happens to enter with no component modifiable by the stress, it will go out in the same condition. Whereas when rotation has taken place inside the glass this constancy of aspect is destroyed, and the light on exit has a different component modifiable by the strain to what it had on entrance.

I do not profess to be able to give a coherent account of how this cause shall give rise to a reduction in the rotation instead of to an elliptical polarization. But then neither am I able to extract from Villari's account of his experiments any assurance that some elliptic polarization was not produced, and that the reduction of the rotation of the polarized plane was anything more than a mixture of small effects not easily analysable nor precisely defined.

It is in any case a most interesting experiment, and should be repeated so as to really get at the bottom of the cause of the observed phenomenon. There are many other experiments on whirling glass which may likewise be made.

APPENDIX, *by* ALFRED LODGE, M.A., *Coopers Hill, Staines.*

The value of

$$\int_0^\infty \sin^2\theta\, dt, \text{ where } \theta = Ae^{-mt}\sin nt, \text{ and } C = \frac{An}{\sqrt{m^2+n^2}},$$

is $$\frac{1}{2m}\left(\tfrac{1}{2}.C^2 - \tfrac{1}{4}.\frac{C^4}{(\underline{|2})^2} + \tfrac{1}{6}.\frac{C^6}{(\underline{|3})^2} - \tfrac{1}{8}.\frac{C^8}{(\underline{|4})^2} + \ldots\right).$$

For

$$\int_0^\infty \sin^2\theta\, dt = \tfrac{1}{2}\int_0^\infty (1-\cos 2\theta)\, dt$$

$$= \tfrac{1}{2}\int_0^\infty \left(\frac{(2\theta)^2}{\underline{|2}} - \frac{(2\theta)^4}{\underline{|4}} + \ldots\right) dt.$$

The general term of this series is

$$\frac{2^{2p}A^{2p}}{\underline{|2p}}\int_0^\infty e^{-2mpt}\sin^{2p} nt\, dt,$$

and can be integrated by successive reduction.

Let u_{2p} denote

$$\int_0^\infty e^{-2mpt}\sin^{2p} nt\, dt\ ;$$

then, integrating by parts,

$$u_{2p} = \frac{-e^{-2mpt}}{2mp}.\sin^{2p} nt\Big]_0^\infty + \int_0^\infty \frac{e^{-2mpt}}{2mp}.2np\sin^{2p-1} nt\,.\cos nt\, dt$$

$$= 0 + \frac{n}{m}\int_0^\infty e^{-2mpt}\sin^{2p-1} nt\,.\cos nt\, dt\ ;$$

and again integrating by parts,

$$= -\frac{n}{m}\frac{e^{-2mpt}}{2mp}\cdot\sin^{2p-1}nt\,.\,\cos nt\Big]_0^\infty$$

$$+\frac{n}{m}\int_0^\infty \frac{e^{-2mpt}}{2mp}\left((2p-1)\sin^{2p-2}nt\cos^2 nt-\sin^{2p}nt\right).n\,.\,d$$

$$=0+\frac{n^2}{2m^2p}\int_0^\infty e^{-2mpt}\left((2p-1)\sin^{2p-2}nt-2p\,.\,\sin^{2p}nt\right)dt$$

$$=\frac{n^2}{m^2}\cdot\frac{2p-1}{2p}\cdot u_{2p-2}-\frac{n^2}{m^2}\cdot u_{2p}.$$

$$\therefore\ \left(1+\frac{n^2}{m^2}\right)u_{2p}=\frac{n^2}{m^2}\cdot\frac{2p-1}{2p}u_{2p-2};$$

$$\therefore\ u_{2p}=\frac{n^2}{m^2+n^2}\cdot\frac{2p-1}{2p}\,.\,u_{2p-2}$$

$$=\left[\frac{n^2}{m^2+n^2}\right]^p\cdot\frac{2p-1}{2p}\cdot\frac{2p-3}{2p-2}\cdots\frac{3}{4}\cdot\frac{1}{2}\int_0^\infty e^{-2mpt}\,dt$$

$$=\frac{1}{2mp}\left[\frac{n^2}{m^2+n^2}\right]^p\cdot\frac{(2p-1)(2p-3)\ldots3\,.\,1}{2p(2p-2)\ \ \ldots\ \ 4\,.\,2}=\frac{\lfloor\underline{2p}}{2^{2p}\lfloor\underline{p}\,\lfloor\underline{p}};$$

$$\therefore\ \frac{(2A)^{2p}}{\lfloor\underline{2p}}u_{2p}=\frac{A^{2p}}{2mp(\lfloor\underline{p})^2}\cdot\left[\frac{n^2}{m^2+n^2}\right]^p=\frac{C^{2p}}{2mp(\lfloor\underline{p})^2};$$

$$\therefore\ \int_0^\infty\sin^2\theta\,dt=\frac{1}{2m}\left(\tfrac{1}{2}\,.\,C^2-\tfrac{1}{4}\cdot\frac{C^4}{(\lfloor\underline{2})^2}+\tfrac{1}{6}\cdot\frac{C^6}{(\lfloor\underline{3})^2}-\ldots\right).$$

Q. E. D.

N.B.—The symbols used for the constants throughout this Appendix have no connexion with those in the main paper, except that m is the same in both. The value of the C of the Appendix, expressed in terms of the symbols used in the paper, is

$$4\pi kn\mu V\omega\sqrt{S\div L}.$$

Mr. Ward mentioned that experiments similar to Villari's were now being carried out at the Cavendish Laboratory, a disk of glass being rotated about 200 times per second by means of a turbine. The results so far obtained do not confirm

Villari's, but owing to difficulties in keeping the speed constant it is impossible to make exact measurements. It has, however, been found that the strain due to centrifugal force rotates the plane of polarization and elliptically polarizes the beam, and that passing an alternate current round a stationary glass bar produces a distinct rotation of polarized light passing through it.

Referring to the oscillatory discharge of a jar, Prof. Rücker directed attention to Dr. E. Cook's experiments, described before the Society in June 1888, when photographs showing the dust-figures produced by sparks were exhibited, and pointed out that the frequency required to produce air-waves of the length there indicated was of the same order as the rate of oscillatory discharge, viz. about 1 million per second. Prof. Rücker also wished to know whether glass behaved precisely like CS_2.

Dr. Lodge said his experiments were not exact enough to decide the latter question, and mentioned that Mr. Chattock had some time ago produced dust-figures in tubes by jar-discharges, and shown that the wave-length depended on the capacity and self-induction.

Prof. Ayrton suggested the use of a phonograph as a means of recording and reproducing the oscillations, in the same way as himself and Prof. Perry have analysed the current curves of alternating dynamos. The discharge could be passed through a small coil fixed to a diaphragm and placed near a coil through which a steady current was passing, the attractions and repulsions serrating the surface of the rapidly revolving cylinder. By means of a mirror attached to a delicate magnifying-spring, the section of the surface may be determined.

Dr. Thompson remarked that it was satisfactory to learn that Villari's results admit of an interpretation other than by time-effect, and thought it advisable to vary the experiment by rotating a bar of glass; but Mr. Ward said he attempted that experiment four years ago, and abandoned it on account of the enormous speed required.

XIX. *On an Electrostatic Field produced by varying Magneti Induction.* *By* Dr. Oliver Lodge *.

In common no doubt with many others, I have long wishe to find some connexion between static electricity and mag netism. My early notions in this direction were such idea as the following :—

(1) To spin a long bar-magnet on its axis, suspending i its field a sort of quadrant-electrometer needle charged oppo sitely at either end, and to look for a deflexion. The iro core of an electromagnet was supposed to serve.

(2) To construct a cylindrical or drum-shaped horseshoe magnet like two iron wheels joined at the nave, and woun with wire there ; to spin this on its axis and suspend a charge pith-ball between the two rims †.

(3) To use a varying magnet instead of a moving on [quite probably this was suggested by Prof. Fitzgerald in conversation I had with him some ten years back] ; fo instance, to suspend a charged gold leaf between the edge of the pole-pieces of an electromagnet, and watch it through microscope when the exciting current is started or stopped.

Many other varieties of the experiment are in my note books, but these are enough to show the idea.

The importance of the experiment seemed to me (rightly o wrongly) to lie in a possible crucial criterion between Max well's theory and some of the contemporary German theories because it seemed to deal with something less than an ope circuit, viz. no circuit at all.

It also struck me as a sort of converse experiment to tha which Prof. Rowland has recently and so successfully carrie out: the classical experiment of deflecting a magnetic needl by a rotating charged disk.

This can hardly be considered a connexion between electr statics and magnetism, because a static charge in motion, not a contradiction in terms, is essentially a current ; an what the experiment proves is the truth of Maxwell's vie that all electricity in motion, no matter whether by conductio

* Read May 11, 1889.

† I find, on reference to Dr. S. P. Thompson's book, that one m describe this arrangement as the field-magnet of a Mordey dynamo.

displacement, or convection, is a true current, and exerts magnetic effects. I should rather say that Rowland's experiment proves the truth of this view of Maxwell so far as convection is concerned. The question of displacement is a ticklish one just now, upon which I shall be safer to be silent*.

But, now, think of a charged gold leaf hanging between the poles of a magnet. If the gold leaf move it constitutes a current, and therefore at once feels a force urging the current (*i. e.* the line of motion) across the lines of force in the orthodox way. Instead of supposing the gold leaf to move, let the magnet move. The same thing will happen, because the relative motion is the same as before. (If absolute motion of the static charge through æther is essential, then this last statement falls flat and may possibly be false.)

If this force were observed, it would be a connexion between magnetism and a static charge, provided any motion of the charged body due to the earth's motion in its orbit were shown to be inoperative or unessential.

Or, instead of moving the magnet, let its strength be varied; lines of force pass by the charged body, unless it is symmetrically situated, in this case as in the other; and hence the effect may naturally be expected to be the same in either case.

The experiment I wished to try was therefore not exactly a converse of the Rowland experiment, but was closely related to it.

Two years ago my friend Mr. A. P. Chattock came to Liverpool to act as Demonstrator, and I found him imbued with similar notions, and also with more practical ideas for carrying them out. Especially was he convinced of the fact that an actual magnetic field in which to suspend the charged body was entirely superfluous, but that varying magnetic induction in a region near the charged body would be equally effective and easier to manage. He wished to use, in fact, a closed magnetic circuit with all its lines enwrapped in itself, and to hang the charged body near but outside the circuit, so

* I say this with reference to what went on at Bath, not with reference to anything that has happened since. This paper was written two or three months ago, before I had heard anything of a communication from Dr. S. P. Thompson to the Royal Society.

that no lines of magnetic force really ever passed through the charge, at least when the magnetic induction was steady. But when the magnetic induction was varied, say by reversing a magnetizing current round the closed magnetic circuit or ring solenoid, Mr. Chattock pictured lines flashing past the charged body in enormous numbers and producing the desired effect.

I am bound to say that the idea of the closed magnetic circuit was not natural to me at first, and I resisted the views of the inventor of the magnetic potentiometer for the best part of a week. At the end of that time my conversion had taken place; and from that time to this some sort of closed magnetic circuit, with one or other form of charged body inside it, has been set up in my laboratory and experiments made with it from time to time.

The theory of the effect I have observed it may be useful just to write down in the most elementary manner.

The magnetic induction in a solenoid being I, the E.M.F. induced in any complete circuit round it, when I changes, is

$$e=\frac{d\mathrm{I}}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If an E.M.F. act on a statically charged body at a distance r from centre of solenoid, the work done in driving it once round a circle is

$$e\mathrm{Q}=2\pi r\mathrm{F}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where F is the mechanical force exerted on the charged body.

Now if the E.M.F. in (2) is the same as in (1), and is caused by the induction I being either generated or destroyed, it will only last a very short time, and we must therefore consider what is the whole impulse of the force during the time it lasts, viz.

$$\phi=\int_0^\infty \mathrm{F}dt=\frac{\mathrm{IQ}}{2\pi r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

And this is the momentum generated whenever the solenoidal magnetism is generated or destroyed. This therefore, or some simple multiple of it, is what is observed in the experiment.

Now in calculating out a numerical value for this we shall write

$$I = \frac{4\pi n CA\mu}{l} = \mu C \text{ times a length,}$$

and $$Q = SV = KV \text{ times a length.}$$

Hence in the product IQ occurs the product μK, and this is the reciprocal of the square of the velocity of light.

This amply accounts for the smallness of the observed effect. By various devices the geometrical parts of I and Q can be increased somewhat to make the observation more easy, but nothing can get over the fact that the velocity of light squared occurs in the denominator of the expression.

To return to the account of experiments devoted to finding this effect. If there is any importance in the result, and I hope there is, though I sometimes fear there is not, much of the credit belongs to Mr. Chattock. The only reason why this is not a joint paper is because he had left the laboratory before a result was obtained.

I must mention also that he showed me, somewhere about this same time, an ingenious paper read before the Society of Telegraph Engineers some two or three years back by Mr. George Forbes, in which that gentleman, making free use of the analogy between a magnetic circuit and a voltaic circuit, predicted almost the precise arrangement which we have found successful, as a magnetic analogue of a tangent-galvanometer.

Some other predictions were also made on the strength of the same analogy; but I have made no experiments like them at present.

The experiment of which I have to speak may be described as follows:—You buy at a shop a large hank of iron wire weighing 28 lbs. or so: you cover it with tape and wind it over with No. 14 copper wire connected to a reversing-key and storage battery. At the centre of the ring you place a glass vessel in which are delicately suspended two oppositely-charged very light conductors, made, for instance, of aluminium foil, connected to each other and to a mirror by a shellac arm. You then work the reversing-key by hand in unison with the natural period of vibration of the suspended arm, so as to keep on reversing the magnetism in the ring, and you hope thus to get up a microscopic swing in the arm or to check an already existing one.

Under these circumstances a result may very well be observed; but it will be certainly due to a spurious cause unless far more elaborate precautions are taken.

For a whole day, last Christmas twelvemonth, I got the most beautifully consistent results. The deflexion reversed with the current, with the sign of the static charge, and everything just as it ought. But before jumping to any conclusion the precaution was taken of going through the operations with the key while one end of the wire was disconnected and the battery circuit therefore not complete. The effect was much stronger.

The whole thing was a mere electrostatic effect due to the action of the slope of potential in the conducting-wire on the very sensitive electrometer-like needle.

To get over this effect was very troublesome and seemed impossible. A number of obvious things were tried, such as cutting the wire in the middle and adjusting its two halves symmetrically; putting the middle of the wire to earth, &c. Putting a carefully selected portion of the wire to earth did, indeed, get over the difficulty; but so sensitive was the arrangement that a variation of an inch in the position of the earth-contact on the length of the wire made a perceptible difference. The effect seemed altogether larger than what we wanted to observe; so, at length, I had the whole glass box containing the charged needle coated with tinfoil.

The result was a beautiful steadiness. The slope of potential was utterly cut off and not a trace of oscillation could be detected on open circuit.

But then neither could anything be detected when the circuit was closed. If the needle were at rest, it remained at rest; and if it were in motion, its motion subsided without the least reference to what one happened to be doing with the key. I came sorrowfully to the conclusion that in cutting off the electrostatic disturbance we had neutralized the thing we were looking for.

But on writing this view to Mr. Chattock, I found he did not agree with it. His view was that the magnetic lines had to get through the space in the centre of the coil, and that, though a sheet of tinfoil might delay them momentarily, it could make no difference in the end; since the time required

for the reversal of the split-up mass of iron was far less than that of a semi-oscillation of the needle.

Not feeling absolutely sure either way, I determined, at any rate, to try a more sensitive arrangement. Hitherto the suspending-fibre had been horizontal, and the time of oscillation fairly quick, the mirror, as it were, lying on its back, and the light-beam reflected down to it by a shaving-glass.

All this was now changed. The iron ring was set up vertically, and the needle was delicately suspended, care being taken to keep its moment of inertia very small. The ring was wound, not all over, but only over a sector of some 45°, and this wire-covered portion was then carefully screened by sheet copper from being able to see the needle and thereby affect it electrostatically. The copper did not form a closed circuit.

The needle was again a pair of scraps of aluminium-foil attached to the ends of a shellac arm holding also a mirror. The charging was done after a proof-plane fashion, and the insulation was pretty good. Asbestos and sulphuric acid kept the air of the box dry.

Taking suitable precautions and trying the magnetic experiment under the new conditions, with the electrostatic effect of the wire carefully screened off, but no other part of the iron ring screened from the needle, an effect was observed, and it was small. But it was irreversible.

I traced it, after a little difficulty, to Foucault currents set up in the aluminium scraps. One might certainly have expected such currents there; but I did not expect them to matter, except by reason of heat, because the metal is in no magnetic field—except, indeed, the earth's. However, it must be that some few stray lines escape from the iron circuit and, crossing through the air, affect the currents excited in the metal by the changing magnetic induction.

So I had to abandon aluminium, and selected very thin mica, silvered chemically, and held curved to the proper curvature by silk threads. But even in the thin silver film Foucault currents could still be detected; so the film was scored regularly all over with a needle-point. But though this diminished the effect to almost nothing, it was still comparable to what we wanted to observe.

So I beat about for other conductors, good enough to charge

statically, but bad enough not to have currents induced in them. At length I hit upon the thin gelatine stuff which is wrapped round crackers. It was not exactly this, but it was some variety of gelatine that Davies ultimately used, constructing it into a pair of little cylinders, say $\frac{1}{8}$ inch long by $\frac{1}{8}$ inch in diameter, very light, and only just conducting enough to receive a charge after they had been in the artificially dried air of the glass box for some time. The shellac arm and everything were made afresh, and as slight and fragile as possible.

I also proceeded to construct a larger and more carefully made iron ring, instead of a mere hank as bought. I thought of making a figure-of-8 ring, winding one loop with wire, and hanging the needle in the other loop; but practical difficulties postponed the adoption of this plan, a circular ring being so much easier; so I went on with the notion of winding the copper wire only over a sector, and screening that by copper sheet. To make this ring an enormous quantity of fine iron wire was procured, and wound for a great part of a month on a suitable wooden mould in a slow-motion lathe, the wire being passed through a flame and through a stick of shellac on its way, so as to roughly insulate its turns, in case we wanted to employ rapid reversals.

The winding continued until the wooden mould began to break down, when it was extracted, all matted together, and mounted on a stone pillar standing on rock in the laboratory.

Some natural difficulty is found in getting the charged needle to hang as wished, with its mirror in the right aspect. Some difficulty is also found in arranging that the stability of the needle shall not be too much increased by its electric charge, so that it oscillates violently about some point and is not properly slow and sensitive.

Another and very tedious difficulty is to arrange so that the position the needle is willing to take up before and after charge is nearly the same; for if the two be very different the spot of light will not keep still, but will sail steadily along as the charge slowly leaks away. To detect small effects the spot of light must be very still. It was difficult to secure this without at the same time applying too severe a constraint, either electric or other, to the needle. It had to be done pretty much by

selecting carefully the shape to be given to the curved plates of the needle, and to the enclosure opposed to them.

All these difficulties were gradually more or less overcome through the patience and skill of my laboratory assistant, Mr. Benjamin Davies.

There remained a few irregular disturbances, some of which could be traced to convection-currents, others to the ordinary movements about a building, and others again to the passage of London and North-Western trains in their tunnel, some 150 yards away and 60 feet down, in the sandstone rock.

The laboratory at Liverpool is very favourably situated as regards shaking. It is a substantial stone building, with walls 2 feet thick, on sandstone rock, which is partially isolated from the railway by the remains of an old quarry, which has been filled with rubble. Street traffic is very distant, and has never been appreciably felt to my knowledge. Students tramping about in the daytime appear to be the cause of most of such irregularities as are felt; but this cause is happily absent at certain hours of the day (or night), and there remains literally nothing but the passage of the North-Western trains, and these are distinctly felt, though only with the most delicate means. The most serious shaking is during a gale. The building then certainly quivers.

To illustrate the unusual steadiness, it may be interesting to mention that Mr. Chattock rigged up the Cavendish Gravitation experiment in my lecture-theatre, taking such precaution as I naturally suggested about suspending from a ball of lead hung from the wall by elastic; and twice I have successfully exhibited the experiment to a fairly large audience, the first time very well indeed. Convection-currents are the real trouble in this experiment, and these can, by elaborate tinfoil screens, be nearly avoided*.

After a few preliminary experiments with the new ring and needle, I began seriously to mistrust the effect of the copper near the ring. Sheet copper was used to screen off the wire

* Since seeing Mr. Boys's beautiful pocket-arrangement for exhibiting the same effect at the Royal Society soirée this week, I am out of conceit with this apparatus. Evidently the experiment is going to become a commonplace of the class-room.

from the needle, and it was also used all over the outside of the arrangement to screen off stray electrostatic actions around. But I fancied I understood now what the effect of such conductors was. The motion of the electricity in them was just of the same sign as the motion of the electrostatic charge for which we were looking ; and if the motion by conduction were permitted, the reaction of the charge thus momentarily redistributed on the conducting metal might just mask the direct effect of the same electromotive force on the charged body ; and would mask it, in so far as it was suitably placed for masking it.

Metals do not screen off the magnetic effect of a moving electrostatic charge (*vide* Rowland), but they might screen off the converse electrostatic effect of a varying magnetic induction.

On the other hand, by arranging a copper conductor so that the effect of electric charges induced in it by the E.M.F. should *assist* the direct effect of the E.M.F. on the charged body, then no doubt a deflexion might be rendered visible.

So I led a short wire round the outside of the ring, and brought its ends into the box where the needle was, one on each side of the needle.

A distinct and proper deflexion was now observed.

A wire was then taken 10 times round the ring, and connected to a common quadrant electrometer. The deflexion was easy to see. It could also be just seen with only one turn of wire.

This kind of experiment seems likely to furnish a method of determining "v"; and it ought to be a good method, because it is v^2 which is really measured.

However, what I had wanted to see was not any effect assisted by the concentration of a conductor, but a result in free dielectric. And this at length has been obtained.

All copper screens and everything of the sort were removed. The ring was wound with only a few turns of thick wire so as to reduce the slope of potential needed to propel a current through it. The wire was wound symmetrically in two halves, and the far ends were connected to a plug-key, so that one might work with either closed or open circuit.

The reversing-key was a quite distinct mercury arrange-

ment specially made for strong currents, fixed to a table on which stood the screen for receiving the spot of light, which was a small disk of translucent paper in a drum of tin plate mounted on an independent stand. The light of a lamp was brought on to the mirror independently of this stand by a 45° looking-glass. Such a screen is handy, because one can follow the rather erratic spot of light all over the table, wherever it chooses to take up its temporary abode.

The middle of the battery was put to earth, so that the potentials of the two halves of the wire might be equal and opposite. The needle was then adjusted by trial and error, its glass box being pushed in and out of the ring, its suspending fibre being turned by a tangent screw, &c., until the electrostatic effect obtained by working the reverser on open circuit was very small. It could never be got quite zero; and even when small it would not always remain small for long.

However, on now closing the circuit by the plug-key, the residual electrostatic effect would be certainly largely reduced and the wished-for effect might be seen. It was seen; the hair-line of the spot of light oscillated over two or three millimetres when the key was worked in unison with the vibrating arm. It assisted the residue of the electrostatic effect when the charging of the needle was in one direction, and it neutralized or reversed the residual electrostatic effect when the charging of the needle was inverted.

It is this reversal of whatever disturbing effect remained which I depend upon as a proof of the correctness of the observation.

Added later (*May* 10).

More recently Davies has noticed a peculiar disturbance when working on open circuit, and when the residual electrostatic effect is very small; viz., that it is not constant in direction. It begins with a kick one way, and then a larger motion the other way. By working the reversing-key quickly one kind of swing can be got up; by working it slowly, the other kind of swing can be got up.

I do not know the meaning of this effect. It is difficult to believe that the fact that the wire is coiled round iron causes

the electrostatic charging of its ends to be slow; though that is the kind of thing it looks like. More probably it is due to some trivial disturbing cause, but the effect is singularly clear and persistent.

To get over the electrostatic effect more completely, and still without the use of a screen, a single layer of copper ribbon was wound on, in about 10 turns, and its outer turn was put to earth.

Another disturbing effect was now noticed: it reversed with the current, and it reversed also when the needle was turned through 180°, but it did not reverse when the charge was simply reversed; nor did it cease when the needle was uncharged. Moreover it was not altogether temporary; there was a residue permanent. Plainly it was a direct magnetic effect.

On taking a map of the field with iron filings, a considerable number of lines of force were found to be leaking across the ring, and these would be able to deflect the needle if it had a trace of permanent magnetization; which it is quite likely to acquire.

It was taken out and tested between the poles of a powerful magnet—the gelatine was distinctly magnetic, the shellac was feebly diamagnetic.

A number of substances were now tried in order to find something sensibly devoid of magnetic properties, and ultimately a paper was found which was barely diamagnetic. With this a new needle was constructed. Wire was once more wound all over the ring, and its field tested with iron filings till hardly any stray lines could be found.

The mirror was removed, and a small pointer supplied instead, so as to be read with a microscope. And in this way the last observations have been made.

The only disturbance now found was due to heat from the wire below the glass box containing the needle, and to get over this Davies rigged up a number of concentric cylindrical jackets of tin plate round the box containing the needle. These certainly constituted an electrostatic screen. Nevertheless, I had hope that though such a screen might diminish the effect it would not destroy it.

I had a letter from Davies yesterday saying that a very

minute effect could be seen with the screens all on, and that it did everything properly as regards reversals. On taking off one screen it increased. On taking off another it increased more, but showed signs of superposed spuriousness. On taking off the third screen the effect was masked by spurious actions.

Everything points to the fact, therefore, that we have now several times observed the true effect, and I entertain practically no doubt of it.

But inasmuch as some of the observations have been made so lately, and some even since I left Liverpool this time, I would not wish the Society to suppose that I regard the research as finished and complete. I should like to clear up distinctly the effect of screens.

I must speak in high praise of the skill and neat-fingeredness of my laboratory assistant Mr. Davies in carrying out the rather troublesome requirements which this finnicking research has at various times seemed to necessitate.

Prof. Fitzgerald, in answer to a question from Dr. Lodge as to the influence of screens, said he had not fully considered the matter in this particular case; and as the general effect of screens depended on the square of "v," the subject required careful treatment. As a means of checking the results obtained by Dr. Lodge he suggested calculating the impulse, and seeing whether its magnitude approximately corresponds with that observed. Commenting on an idea for carrying out a similar experiment attributed to him in the paper, in which a charged gold leaf is placed between the poles of a magnet, Prof. Fitzgerald said he had been misunderstood, for he had conceived a disk parallel to the faces of the magnets which, when excited, should cause the disk to turn in its own plane.

Referring to the equations for mechanical force given in Maxwell, § 619, he pointed out that the coefficient of e in the equation $X = cv - bw - e\frac{d\Psi}{dx} - m\frac{d\Omega}{dx}$ ought to be P, where $P = c\dot{y} - b\dot{z} - \frac{dF}{dt} - \frac{d\Psi}{dz}$, and considered it very important that the existence of the term $e\frac{dF}{dt}$ should be tested experimentally.

Prof. S. P. Thompson mentioned some experiments on which he was engaged by which he hoped to show electric displacement in continuous dielectric circuits, such as a hank of gutta-percha. Up to the present the experiments had not been successful, owing to his inability to place the two gramme ring-coils used into such relative positions as to give silence in the telephone connected with the coil used as secondary, when currents were sent through the primary.

Prof. Ayrton suggested that Dr. Thompson's difficulty may arise from the fact that such rings do produce considerable external field even when carefully wound.

Prof. Fitzgerald requested Dr. Thompson to investigate the effects of displacement-currents and of changing vector-potential, and pointed out that in a single medium the former can produce no magnetic effect. As regards fields containing different media, he said the calculations would be complicated by the spurious charges on the separating surfaces.

Dr. Lodge, in reply, said he had calculated the momentum to be expected in one arrangement of his experiment in which a suspended aluminium cylinder surrounds one limb of a rectangular magnetic circuit which formed the core of an induction-coil; one end of the secondary was put to the core and the other to the cylinder, thus forming a condenser. The result came out about 10^{-6} dyne second, but he could not say whether such a small quantity was observable.

XX. *Quartz as an Insulator.* *By* C. V. Boys, *A.R.S.M., F.R.S., Assistant Professor of Physics at the Normal School of Science, South Kensington**.

[Plate II.]

When making quartz threads by the bow-and-arrow process described in the Philosophical Magazine, June 1887 (Proc. Phys. Soc. vol. ix. pp. 8–19), I have sometimes noticed that the thread does not reach all the way from the arrow to the bow, but that the end remains suspended in mid air somewhere between. When this is the case the last foot about, then very fine, is usually in the form of an irregular helix. Under these circumstances, if the hand is brought at all near the end the helix stretches itself out, and the end

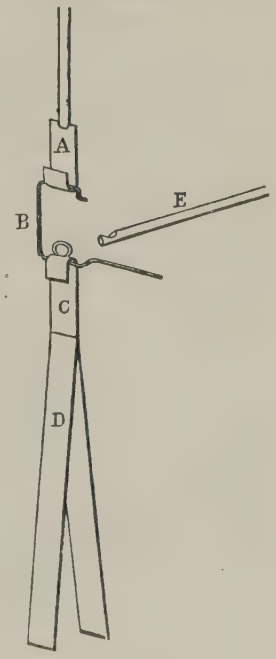
* Read April 13, 1889.

of the thread flies to and attaches itself to the hand. On removing the hand the thread takes its old form; and this may be repeated several times.

It did not seem possible to account for this in any way except by supposing the thread to be electrified, though why it should be electrified is not clear. If this is the case then the insulating-power of the thread must be very great, for with the very small quantity of electricity which could remain on a body of such immeasurably small capacity, all trace of charge would escape instantly if the thread insulated no better than glass in the open air.

I therefore thought it would be interesting to see if rods of fused quartz showed any great superiority over similar rods of glass under the same circumstances. The plan that I have followed has been to hang a pair of very narrow gold leaves from the rod under examination, and observe the rate at which they closed after being charged. I purposely avoided all instruments the large capacity of which would increase the time of discharge, and the leakage of which might be comparable with or even exceed that of the rods to be tested.

Fig. 1.



The arrangement of leaves &c. is shown in fig. 1. A flat brass hook, A, is fastened to a rod which can slide stiffly through the centre of the lid of a mahogany box lined with tinfoil. From this is suspended the piece of glass or quartz, B, bent to the form shown, so that it can be handled by the projecting end without touching the portion that acts as the insulator. On this hangs a piece of bent brass, C, to which the leaves, D, are attached. A small ring of wire is soldered to the upper end of C, which is used when one insulator is changed for another as follows:— A stiff wire, E, passes through one side of the instrument, and this is pushed forward through the wire

ring. A is then depressed until the lower side of B hangs clear of the hook C. E is then drawn back with C and D suspended from it. Finally, when B is changed, E is pushed forward again, and A raised until the ring on C is just free from E, which is then withdrawn. By this means the leaves are always left at the same level. The leaves are 90 × 5 millim., and the box, which has a glass front and back, is 285 millim. high × 130 millim. wide, and 185 millim. from front to back. The leaves are suspended so that the line of junction of C and D is 160 millim. above the base of the instrument. The length of the insulating portion of the quartz and glass hooks B is 21 millim. and the diameter about 1 millim. The leaves were observed by fixing the object-glass of a telescope at a distance of 393 millim., which projected an image of the leaves on a scale 1940 millim. beyond the lens. Thus the observed divergences were 4·93 times the true distances between the ends of the leaves.

The results of the experiments can be seen from fig. 2 (Plate II.). The rate at which the leaves close is the same with lead-glass in air dried by sulphuric acid, with quartz in air dried by sulphuric acid, and with quartz in air kept moist by means of a large flat dish of water. Soda-glass in air dried by sulphuric acid allowed the electricity to escape about eleven times as fast. With either kind of glass in moist air the charge escaped almost at once, but soda-glass was much worse than lead-glass. The glass had in all cases been boiled in distilled water, a process which Warburg and Ihmori* have shown is necessary in order to make the glass insulate as well as possible. There is no appreciable difference between the rate at which positive and negative electricity escapes from the leaves.

The quartz insulator was then treated in various ways to see how well it is likely to retain its insulating power. It was boiled for five minutes in a weak solution of potash and washed. It was boiled for the same time in a strong solution of potash and washed. In both cases it insulated as before. It was dipped for two minutes in melted potash and washed. In moist air it insulated better than either soda or lead-glass, but not so well as before treatment with potash. Boiling in strong hydrochloric acid did not restore the lost power. A

* Wied. *Ann.* xxvii. p. 481 (1886).

new hook was not affected by boiling in strong hydrochloric acid, or by heating in a batswing gas-flame.

Perhaps the most surprising result is obtained by dipping the quartz hook in water or ammonia, and immediately hanging on the leaves while the water is standing upon the hook in beads. Even so no difference is observed in its insulating-power. If it is dipped in a solution of potash this is not the case ; but of course the insulation is restored by washing.

The perfect equality of the rate at which the charge escapes when the leaves are suspended from lead-glass in dry air, or quartz in dry or moist air, makes it probable that this loss of charge is not due to leakage along the insulator, for it is very unlikely that, under these different circumstances, the loss should be exactly alike. It is more probable that the loss is due mainly to convection through the air. This is made certain by the following considerations. The same leaves, when hung by the same hook in another box which was badly made and rough inside, lost their charge much more quickly, but, as before, at the same rate in the three cases. On the other hand the leaves, when suspended in the best instrument by a quartz fibre about ten times as long and one hundredth of the diameter of the piece B (that is, by one which would insulate a thousand times as well if the loss were due to surface creeping, or a hundred thousand times as well if it were due to actual conduction), lost their charge practically at the same rate as before.

Pieces of polished rock-crystal, such as are used as objects for the polariscope, also insulate well ; but they do not seem to be quite so free from the influence of moisture as the fused quartz. The same is true with regard to the natural faces and the fractured surface of the crystal.

The electromotive forces required to produce different divergences of the gold leaves were determined by an absolute electrometer, and the results are shown in fig. 1 (Plate II.).

It is probable that this valuable property of quartz, that it insulates perfectly in damp air, may be of use in the construction of electrostatic apparatus. The sulphuric acid now absolutely necessary in electrometers and instruments of that class is nothing short of a nuisance. If the instrument is carried about there is the risk of destruction of the instrument

from the spilling of the acid. If the instrument is not moved, the acid, unless specially treated, may give off nitrous fumes which will corrode the surfaces of metal; or, if forgotten, it absorbs water and in time overflows, destroying the whole apparatus. Even if the air were saturated with moisture, rods of quartz would insulate as well as the lead-glass at present used does in air dried by sulphuric acid. The needle should of course be suspended by a fibre of quartz, which is far simpler to apply and adjust than the double line of silk, and superior also in other respects.

In conclusion I must express my obligations to Mr. Briscoe, a student in the laboratory, whose skill in the manipulation of gold leaf and whose suggestions from time to time have been of the greatest service. I have with perfect confidence asked him to carry out the experiments described in this paper, and the results show that the confidence was not misplaced.

XXI. *Water-spray Influence-Machine.*

By GEORGE FULLER*.

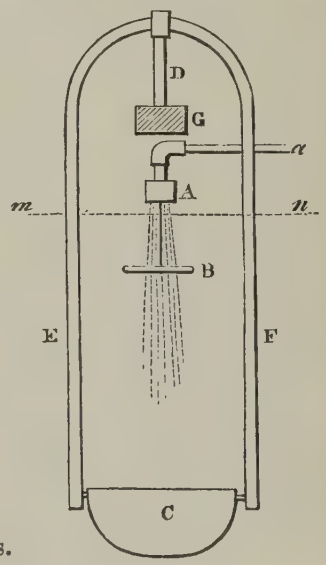
THIS machine is for obtaining directly from a fall of water a supply of electricity of a high potential. It consists of four similar parts arranged symmetrically round a central vertical support, and each division has the following members.

A nozzle, A, in connexion with a head of water by means of a pipe, *a*.

A ring, B, of brass or copper wire placed vertically below A, and through which the water descends when the machine is in action.

A vessel, C, placed below B to receive the water that has passed through the ring.

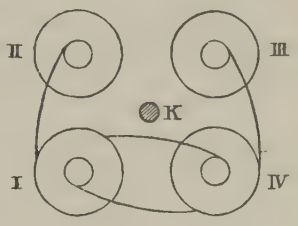
A brass tube, E F, between the ends of which the vessel C can turn about a horizontal axis.



* Read May 25, 1889.

An insulating glass rod, D, to the top of which the tube E F is attached, and with the lower end fixed in a part of the frame of the machine, G.

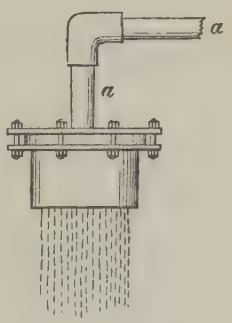
A sectional plan on *m n* shows the connexion between the four divisions, which are numbered I., II., III., IV. The wire ring of section I. is in electrical connexion with the receiver of section IV. Similarly the ring of II. is connected with I., the ring of III. with IV., and that of IV. with I.



K is a central column for supporting four arms of the machine to which are fixed the insulators D.

The discharge of electricity is taken between conductors in connexion with II. and III.

The nozzle is a flanged brass box, the bottom of which is perforated with small holes through which the water descends. It is fixed by a number of small bolts and nuts through its flange to a brass plate fixed to the supply-pipe, *a a*, and the joint is made watertight by a vulcanized india-rubber ring. A piece of fine linen covers the top of the box to strain the water before it reaches the small holes, as it was found that the water was either stopped or diverted by small particles unless this precaution was taken. The holes, which are circular, have a diameter of $\frac{1}{100}''$; as it was found that when holes $\frac{7}{1000}''$ were used the water was so much dispersed by the working of the machine that a great part of the water ceased to fall into the receivers. This great dispersion also injured the insulation, and besides this it was extremely difficult to keep these holes free.

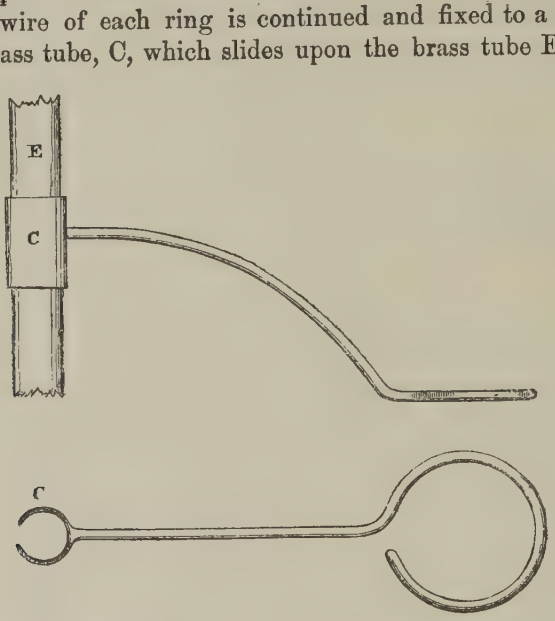


With regard to the number of holes. In the nozzles of sections I. and IV. there are six arranged in a circle of $1\frac{1}{4}''$ diameter. For those of sections II. and III., either a pair

with twelve holes each in a circle of $1\frac{1}{4}''$ diameter, or with eighteen holes in a circle of $1\frac{3}{4}''$ diameter.

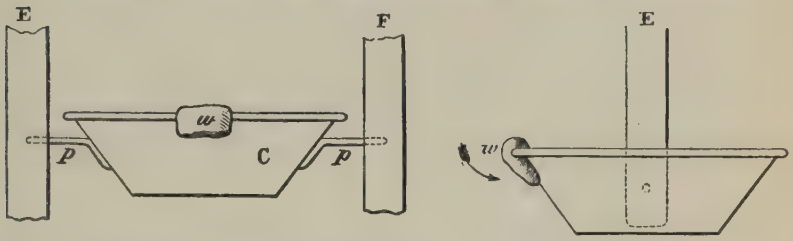
The rings are made of brass or copper wire of about $\frac{1}{8}''$ diameter. The inside diameter of the rings used with the nozzles with six and twelve holes is $2\frac{1}{4}''$, and with the eighteen holes $2\frac{3}{4}''$.

The wire of each ring is continued and fixed to a clip of split brass tube, C, which slides upon the brass tube E or F.

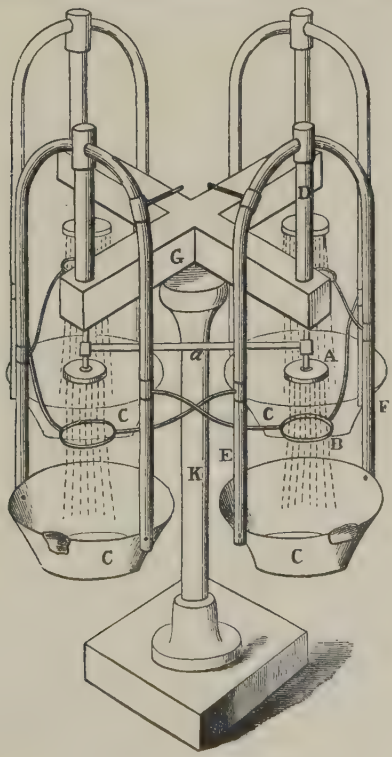


This enables the depth of the ring below the nozzle to be adjusted, which is of importance, as the greater the head of water employed the greater must be the distance between the two, as the ring should be fixed at the point where the small streams of water break up into spray. The receiving vessel, C, may be of glass or metal, as the former material, from its constant state of moisture whilst the machine is working, seems to conduct the electricity as effectually as the latter. In the author's model they were at first of glass, but one of them having been broken they were replaced by receivers of zinc. The receivers are supported by pins, p, p, which are soldered to them at one end, whilst their free ends rest in holes drilled in E and F. To make the receiver self-acting they are hung so that when a leaden weight, w, is fixed, as

shown in sketch, the vessels being empty, they would turn in the direction of the arrow if they were not prevented by stops soldered to them which press against E and F; but when



the receivers are nearly full of water, their balance is such that they turn in the opposite direction and so empty themselves. The four receivers are made to turn towards the axis of the machine and to deliver the water into a metal bath, which for continuous action should be connected with a drain.



The following are some of the dimensions of the author's model :—

Zinc receivers 8″ diameter at the top.

Brass tube E and F $\frac{3}{8}$″ diameter.

Glass rod D $\frac{3}{4}$″ diameter, with an insulation of 4″.

From the rim of C to the lower surface of A, $9\frac{1}{4}$″.

From the rim of C to the underside of stand, 1′ 1″.

From centre to centre of insulators D across the centre line of instrument, 1′ 1″.

The Electrical Action of the Machine.

An instrument made of only sections I. and IV., with their rings connected with their receivers, as shown above, will charge itself; and the difference of potential of the two receivers may be such that sparks $\frac{1}{2}$ inch long may occasionally pass between them, though more usually $\frac{3}{8}$ inch is the longest that can be obtained with a head of water of about 23 feet. With this arrangement, after every discharge the potential of the rings is nearly equalized; whereas in the machine with four sections, I. and IV. keep up the difference of potential of the rings of II. and III.

With respect to the action of the machine, the author, whilst giving the considerations from which it was constructed, must leave to the electrician to determine whether they have anything to do with the true explanation of the phenomena. The water, at the point where it is divided into drops by the resistance of the air, is electrified by induction from the rings; the former being in connexion with the earth through the unbroken water of the stream, and the action seems similar to that employed in Sir W. Thomson and Professor Silvanus Thompson's water-dropping accumulators. That such is the case appears to follow from the fact that, if the rings are either placed much above or much below the level where the water breaks into spray, the machine ceases to work. When the rings are at their proper level there is an additional action; for the particles that are inductively electrified are split up into numberless minute particles, some of which are so fine that they float about in the air and do not fall into the receiver. And it is this breaking up of the water into minute particles that the author thinks may account in part for the

effect produced ; for when a number of spheres that have been electrified unite into a mass of less surface, their potential in the latter state is higher than in the former.

Another point which the author thinks must be taken into consideration is the speed with which the particles move through the ring, as it was only when he experimented with a fall of some feet instead of inches that he obtained a potential high enough to produce sparks. With a very slow speed the attraction of the ring is too strong for the water, so that it at last, as in Sir William Thomson's apparatus, bends against it. That the division of the drops into minute spray plays a part in the action of the machine seems to be shown by the fact that sparks of the same length, in the same state of the atmosphere, have been obtained from it when the velocity of the water has been very much diminished. The sparks, as a rule, have not been so numerous per minute, but the water has been divided into finer spray. At times, even with half the delivery of water, the same length of spark has been obtained.

One experiment the author has made in which the spray was not obtained by the action of gravity, but by a steam "atomiser," as it is called. The water and steam passed through a copper wire ring $1\frac{1}{2}''$ diameter, connected with one of the receivers of an apparatus made up of sections I. and IV., as above. The nozzle was $\frac{5}{8}''$ from the ring and $5\frac{5}{8}''$ from the receiver. Sparks $\frac{1}{4}''$ in length were taken freely from the receiver, which is a better result than has been obtained with a fall of water of some 23 feet. What was very observable in this case was the very small amount of water used, a small teacup-full being passed over in some five or six minutes ; and the author has recorded in his notes that the experiment was made on a very wet day.

Adding to the number of jets does not seem to increase the power of the machine, either in quantity or potential, at all in proportion to the number added ; though the action of an electrical machine is so eccentric that it is difficult to be certain of this, for at times the nozzle with eighteen jets has given much better results than the one with twelve jets.

It has been stated that, in the machine as made, the rings are $1''$ larger in diameter than the circle of the jets, and it is

found that they give a better result than when larger rings are used; but in some experiments with a small flow of water a ring $3\frac{1}{2}''$ diameter gave as large a spark as one of $1\frac{1}{2}$. In the dark, electricity is often seen to fly off from the rings, the water on them being made into pointed-shaped drops.

The machine in its present form is by no means powerful, as with a small Leyden jar attached to it the longest spark has hitherto been $1\frac{1}{8}''$, the head of water being about 23 feet. The state of the atmosphere has very great influence on the working of this machine; for though in all states of the weather electricity will be generated, it requires a fairly dry atmosphere to give $1''$ sparks.

It may be mentioned that the machine has only been tried in a small bath-room, which is a very unfavourable place for electrical experiments; and it perhaps is worth mentioning, that on one occasion sparks were only obtained when window and door were open and the machine was in a thorough draught.

To what extent the power of the machine may be increased it is difficult to predict; but the author thinks that the experiment with the atomiser points to high velocity in the water, combined with minute subdivision, as the direction in which any future attempts should be made.

Prof. S. P. Thompson enquired whether the length of the spark was limited by leakage along the glass rods or by the spray passing between the receivers, and in reply Mr. Fuller said he thought the former leakage the most important.

XXII. *On the Molecular Weight of Caoutchouc and other Colloid Bodies.* *By* J. H. Gladstone, *Ph.D., F.R.S., and* Walter Hibbert, *F.I.C.**

During the last meeting of the British Association at Bath, we gave a preliminary account of some attempts to determine the molecular weights of caoutchouc and a few other substances by Raoult's method. We have since repeated most of the experiments and largely extended the inquiry, and it seems to us that the results have a certain physical as well as chemical interest.

It is evident that this method is the only one that offers

* Read May 25, 1889.

much hope of success in dealing with such substances as caoutchouc, but it is open to question how far the method itself is to be trusted for giving the correct molecular weight of compounds of this description. Our confidence in it, however, was strengthened by the following experiments, made on substances of the same ultimate composition ($nC_{10}H_{16}$), but of known molecular weight in the gaseous condition. We also made experiments on one or two closely allied bodies containing oxygen.

The compounds were dissolved in benzene which had a freezing-point of 5°·25 C., and the experiment was conducted in the usual manner. Each degree of the thermometer scale was divided into twentieths, and it was not difficult to estimate to the hundredth of a degree. Successive observations of a freezing-point nearly always agreed to less than 0°·02.

The following table gives:—in col. II. the recognized molecular formula, in col. III. the strength of solution, in col. IV. the amount of depression, and in col. V. the molecular weight calculated by Raoult's formula $M = \frac{T}{A}$, where T is the molecular depression constant (in this case = 49), and A is the depression given by 1 gram of the substance in 100 grams of solvent. These figures may be compared with col. VI., which gives the molecular weight deduced from the formula in col. II.

Substance.	Col. II.	Col. III.	Col. IV.	Col. V.	Col. VI.
		per cent.	°		
Oil of Turpentine.....	$C_{10}H_{16}$	4·56	1·59	140·5	136
Oil of Lemon.....	„	6·04	2·17	136·4	136
„	„	3·06	1·12	133·8	„
Cedrene	$C_{15}H_{24}$	3·89	1·00	190·8	204
„	„	4·71	1·20	192·3	„
Isoprene	C_5H_8	3·30	2·25	71·9	68
„	„	2·20	1·52	70·9	„
Caoutchene	$C_{10}H_{16}$	5·38	2·01	131·1	136
Heveene.....	$C_{20}H_{32}$	12·00	2·32	275	272
„	„	9·37	1·85	248	„
„	„	7·68	1·53	246	„
Camphor	$C_{10}H_{16}O$	4·69	1·59	144·5	152
Menthol.....	$C_{10}H_{20}O$	3·21	0·93	169·1	156
„	„	4·93	1·31	184·4	„
„	„	3·75	1·07	171·7	„
Anethol	$C_{10}H_{12}O$	3·71	1·29	141·0	148

This table shows not merely that the method is applicable in the case of bodies of this description, but that the molecular weights of the liquids in solution have the same relative values as in the gaseous condition.

We then made experiments on caoutchouc, whose empirical formula as usually given ($C_{10}H_{16}$) would indicate a molecular weight of 136, and we found that this was very far below that deduced from our results, as shown in the following table:—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight.
Caoutchouc (*a*) ...	3·1	Scarcely observable.	Extremely high.
„ (*b*) ...	8·8	„	„
„ (*c*) ...	14·6	0·11	6504

The caoutchouc used in solution (*a*) had been prepared from Penang rubber, by the process described in our previous paper (Chem. Soc. Journ., July 1888, p. 679). That in solution (*b*) was obtained from Para rubber, by dissolving it in ether, and precipitating the etherial solution with alcohol. Solution (*c*) was prepared from (*b*) by evaporation in a current of hydrogen. The greater depression observed can hardly be ascribed solely to the greater strength of the solution, since that would only give a proportionate effect. We are inclined to think it possible that there was a lowering of the molecular weight during a three days' gentle heating which was incidentally necessary. The observation, in fact, seems in harmony with other alterations in physical properties which we have sometimes noticed.

This very high molecular weight for caoutchouc strengthens a previous impression of ours that caoutchouc belongs to the class of substances known as colloids. The impression arose from the fact that caoutchouc is a substance showing not the least tendency to crystallize, which cannot be distilled without decomposition, which is subject to great alteration of properties by the action of heat, which is converted into an insoluble modification by small quantities of certain reagents,

and which dissolves in its solvents in an extremely sluggish manner.

Graham, in his classic memoir on the subject* of Colloids, observed that "the equivalent of a colloid appears to be always high;" and he also suggested that the colloid molecule may be "constituted by the grouping together of a number of crystalloid molecules."

It seemed worth while therefore to examine bodies commonly regarded as colloidal by Raoult's method. The following table gives the results obtained with aqueous solutions of organic colloids, the molecular weights being reckoned for the ordinary value for T given by Raoult in the case of water :—

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. T=19.
Gum arabic	31·6	0·3°	2001
Ditto purified ...	14·0	0·165	1612
Caramel.....	8·76	0 105	1585
„	22·5	0·245	1745
Albumen	2·0	Scarcely observable.	Extremely high.

The molecular weight of these known colloids, as determined by Raoult's method, is very high and confirms the generalization of Graham.

Experiments have already been made upon the so-called carbohydrates by this process by Messrs. H. T. Brown and G. H. Morris†. They found that the sugars had a molecular weight agreeing with the received formula, but the noncrystallizable bodies like soluble starch &c. gave them results suggestive of specially high molecular weight.

We may also note that in some recent investigations by C. Lüdeking, he found that the addition of colloids to water makes no practical difference to the boiling-point, and in every case lowers the vapour-pressure very slightly‡. These results all indicate the same general conclusion.

* Phil. Trans. 1861, pp. 183-224.

† Chem. Soc. Journ. 1888.

‡ *Ann. Phys. Chem.* [2] xxxv. pp. 552–557.

Our experiments were extended by making an examination of solutions of the colloidal hydrates of aluminium and iron. They were prepared by dialysing solutions of the basic chlorides, but, as is well known, a small proportion of the salt must be retained in order to prevent coagulation. The iron solutions contained almost exactly one molecule of chloride to fifteen molecules of the hydrate. The first aluminium solution contained one molecule of the chloride to five or six of the hydrate, the second one of chloride to nearly ten of the hydrate.

Substance.	Weight in 100 grams of Solvent.	Depression.	Molecular Weight. T=47.
Ferric Hydrate.....	1·16	About 0·01	5452
„	2·60	0·025	4888
Aluminic Hydrate...	0·523	0·060	409·6
„	1·37	0·06	1073·0

The figures here given for the molecular weights of the hydrates are calculated as if the whole depression were due to the hydrate in solution, but the chloride present must have exercised a considerable influence, especially in the first aluminium solution. If allowance be made for this, the molecular weights found would be higher than those given in the table, and would point to the soluble colloidal hydrates of iron and aluminium being many multiples of $Fe_2H_6O_6$, or $Al_2H_6O_6$, which would give a molecular weight of only 214 and 157 respectively. The molecular weights of ferric and aluminic chlorides, as determined by Raoult's method (T being 47), are about 114 and 106 respectively.

All our experiments, therefore, while affording additional illustrations of the value of Raoult's method, confirm the belief that the molecule of a colloidal substance is an aggregate of a very great number of atoms*.

* Since this paper was read we have found that Paternò and Nasini have arrived at the same conclusion from experiments on albumen and gelatine (*Lincei*, April 7, 1889, p. 476).

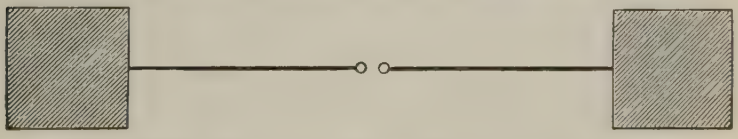
XXIII. *On Electric Radiation and its Concentration by Lenses.* *By* Prof. Oliver J. Lodge, *D.Sc., F.R.S., and* James L. Howard, *D.Sc.**

Introduction by Dr. Lodge.

In making exact optical experiments on electric radiation it is necessary to be able to converge it and throw a beam of it in any desired direction. To do this by means of mirrors is possible, but not always very convenient. Prof. Fitzgerald and Mr. Trouton† have related the difficulty they at first found in making concave mirrors work; and we experienced the same difficulty, intensified probably in our case by the fact that we tried to work with everything on an extra small scale—half the linear dimensions of Hertz.

It is much easier to work with a large oscillator than a small one, because the same extraordinary suddenness in starting the oscillations is not then essential; only with large waves, mirrors and everything have to be heroic to match, and our laboratory was not big enough for optical experiments on gigantic waves. Electrical experiments on such waves I have made in large numbers, obtaining them originally by means of discharging Leyden jars, but recently sometimes by a gigantic Hertz oscillator consisting of a pair of copper plates, each consisting of a couple of commercial sheets soldered together and rimmed round with wire, connected by a length of No. 0 copper wire interrupted in the middle by a couple of large knobs. The plates and connecting-rod are hung from a high gallery, so that everything occupies one plane, their distance and dimensions being here shown.

Fig. 1.—Large Oscillator used for violent and distant effects. Scale $\frac{1}{80}$.



Plates 120 centim. square. Knobs 3·2 centim. diameter.
Each rod 230 centim. long and 8 millim. diameter.
Spark-gap about 1·5 centim.

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* Read May 11, 1889.

† 'Nature,' vol. xxxix. p. 391.

Static capacity, $\frac{S}{K}$ =25 centim.

Self-induction, $\frac{L}{\mu}$ =8320 „

Characteristic factor, $\log\frac{4l}{d}$ =7·9.

Rate of vibration, 10 million per second.
Wave-length, 29 metres.
Dissipation-resistance, 22,500 ohms.
Initial stock of energy, about 300,000 ergs.
Power of initial radiation, 128 horse-power.
Number of vibrations before energy would be *at this rate* dissipated, about 3.

The electrical surgings obtained while the Hertz oscillator is working are of just the same character as are noticed when a Leyden jar is discharging round an extensive circuit; but whereas from a closed circuit the intensity of the radiation will vary as the inverse cube of the distance as soon as the circuit subtends a small angle, the radiation from a linear or axial oscillator varies in its equatorial plane only as the inverse distance, as Hertz showed.

Hence, for obtaining distant effects the linear oscillator is vastly superior. Its emission of plane-polarized, instead of circularly-polarized, radiation is also convenient.

(I may mention that a thundercloud and earth joined by a lightning-rod or by a disruptive path constitute a linear oscillator; and hence radiation-effects and induced surgings may be expected to occur at very considerable distances from a lightning-flash.)

Exciting this oscillator by a very large induction-coil, extraordinary surgings are experienced in all parts of the building, and sparks can be drawn from any hotwater-pipe or other long conductor, whether insulated or otherwise, and from most of the gas-brackets and water-taps in the building, by simply holding a penknife or other point close to them. From conductors anywhere near the source of disturbance the knuckle easily draws sparks.

Out of doors some wire fencing gave off sparks, and an iron-roofed shed experienced disturbances which were easily detected when a telephone-terminal was joined to it, the other terminal being lightly earthed. [Sometimes I utilized the

wire fencing as one of the plates of the oscillator, and thus got still bigger and further spreading waves.]

The waves thus excited are from 30 to 100 yards long, and optical experiments with them would be as difficult and vague as are experiments on sound-waves of corresponding length. Small oscillators can, however, easily be employed which shall give waves from a foot to a yard in length; and after reading Hertz's experiment of the pitch prism*, I made preparations for casting some great lenses that should give, I hoped, easy concentration of such waves.

Paraffin was a natural substance to use; but it is rather expensive, and has not a very high index. After considering many substances—beeswax, sulphur, &c.—I decided to try resin, and laid in a stock of that substance. Meanwhile, to gain experience in casting, and finding that a common class of pitch could be obtained at an absurdly low price, I procured several casks of the commonest pitch also. I did not contemplate using this substance at first because I feared it would be an imperfect insulator, and there seemed no use in permitting any dissipation of energy whatever, so long as one could get perfectly transparent substances.

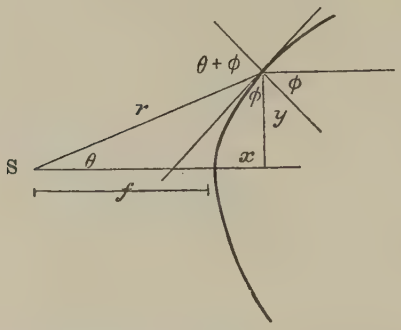
On casting a specimen of the pitch, however, it was found so strongly insulating as nearly to fling off the leaves of a gold-leaf electroscope it was brought near. It seems, therefore, an excellent cheap stuff for electrophorus and such like use, wherever it is not expected to be strictly solid; and it can hardly help being transparent except to very little waves.

Meanwhile we had calculated that to receive rays from one point and convey them to another without aberration, a pair of plano-hyperbolic lenses were very suitable; a parallel beam being transmitted from one lens to the other. The lenses would naturally be made cylindrical, instead of spherical, to suit the linear form of radiator.

The optical calculation of a lens free from aberration for one special point, S, from which it is to receive rays and emit

* Wied. *Ann.* xxxvi. p. 769 (1889); translated in Phil. Mag. April 1889.

Fig. 2.



them parallel, is as follows :—From fig. 2,

$$\mu = \frac{\sin(\theta+\phi)}{\sin\phi}\ ;\quad y=(x+f)\tan\theta\ ;\quad \frac{dy}{dx}=\cot\phi.$$

Solving these equations, and making θ and x vanish together, we get, as the curve of the lens,

$$r=(x+f)\sec\theta$$

or

$$r=\frac{f(\mu-1)}{\mu\cos\theta-1};$$

a hyperbola with one focus as origin, with eccentricity μ, semi-latus rectum $f(\mu-1)$, and semi-axis major $\frac{f}{1+\mu}$.

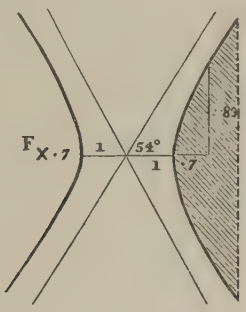
Taking μ as 1·7 for pitch (according to the measurement of Hertz with a prism), and calling the semi-axis major unity, the focal length of the lens is 2·7, the semi-latus rectum 1·89, and half the angle between the asymptotes, being $\cos^{-1}\frac{1}{\mu}$, is 54°.

Using these data, and taking six inches as unit of length, a curve was drawn as shown in fig. 3, where F, the focus of the hyperbola, is to be also the principal focus of the lens ; its distance from the lens is 41 centim.

This curve was given to the laboratory assistant, Mr. Davies, who cut out a pair of wooden templates to the pattern, nailed sheet zinc to them so as to make a mould, propped it up

in an outhouse, and proceeded to cast it full of pitch—the upper fluid surface constituting the plane surface of the lens.

Fig. 3.



All went well till the mould was nearly filled, when the weight of the pitch ripped the zinc from its fastenings, and a horrid collapse was the result.

A couple more moulds were made of the same pattern, only stronger, and a bed of sawdust and mould was made to sustain the weight. A double partition of thin wood was introduced across the middle of the mould, so as to enable each lens to be split into two halves if it should happen to be too immovable in one piece.

After a time two satisfactory lenses were obtained, each nearly a metre square. Nothing could be done with them during term, because of want of space; but in the Easter holidays I requested my demonstrator, Dr. Howard, to make experiments in one of the College corridors. There exists a large open room or iron shed, which I should have preferred to use; but unfortunately dry rot had set in in its flooring, and it was in the hands of the carpenters all vacation. We are therefore somewhat troubled by neighbouring walls and by hotwater-piping.

Under more favourable circumstances, the distance between the lenses might no doubt have been much greater; in fact, no attempt was made to place the lenses far apart. They were set up with their flat faces parallel at the opposite ends of a table, about 6 feet apart, and not afterwards moved,

being, indeed, rather unwieldy; the oscillator was placed in the principal focus of one lens, viz. at a distance of from 41 to 51 centim. from its curved surface. The focal length, calculated on the assumption that $\mu = 1{\cdot}7$, was 41 centim., but experimentally 51 centim. seemed to do better.

After the few experiments here recorded were done, one of the lenses took advantage of Easter week to assert its essential fluidity, and so much bulged and curved over as to be almost unserviceable; since then it has completed its ruin by breaking its prop and tumbling over into fragments. The other lens stands remarkably well, and seems as good as ever. There is evidently an important difference in the quality of the pitch, though it is not a difference recognized by the invoice. On the whole I think paraffin would have been the best substance to use.

The particular form of receiver is a comparatively unimportant matter, but I prefer linear ones to circular or nearly closed circuits as being more sensitive at great distances, for much the same reason as has been stated for oscillators.

Exact timing of the receiver is unessential. If resonance occurred to any extent, so that the combined influences of a large number of vibrations were really accumulated, the effects might doubtless be great; but hitherto I have seen no evidence of this with linear oscillators; the reason being, I suppose, that the damping out of the vibrations is so vigorous that all oscillations after the first one or two are comparatively insignificant; and very bad adjustment, or no adjustment at all, will give you the benefit of all the resonance you can get from such rapidly decaying amplitudes. The main reason of the rapid damping is loss of energy by radiation. The "power" of the radiation while it lasts is enormous, and the stock of energy in a linear oscillator is but small.

Leyden-jar discharges in closed circuits die away more slowly, and for them some approach to exact timing is essential, if a neighbouring circuit is to respond easily.

In working with small oscillators it is essential that the spark-knobs shall be in a state of high polish, else the sparks will not be sufficiently sudden to give the necessary impetus to the electrification of the conductors.

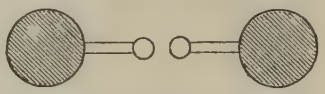
Any hesitation or delay about the spark permits the potentials of the knobs to be equalized by a gradual subsidence which is followed by no recoil, just as a tilted beer-barrel may be let down gently without stirring up the sediment by waves. The period of a natural vibration is comparable to the time taken by light to travel a small multiple of the length of the oscillator, and hence not a trace of delay is permissible in the discharge of a small conductor if any oscillations are to be excited by means of it. Thus if an electrostatic charge on a conducting sphere be disturbed in any sudden way, it can oscillate to and fro in the time taken by light to travel 1·4 times the diameter of the sphere, as calculated by Prof. J. J. Thomson; and hence it is by no means easy to disturb a charge on a sphere of moderate size except in what it is able to treat as a very leisurely manner. Even on large spheres the oscillations cannot be considered slow: thus an electrostatic charge on the whole earth would surge to and fro 17 times a second. On the sun an electric swing lasts $6\frac{1}{2}$ seconds. Such a swing as this would emit waves 19×10^5 kilometres or twelve hundred thousand miles long, which, travelling with the velocity of light, could easily disturb magnetic needles* and produce auroral effects, just as smaller waves produce sparks in gilt wall-paper, or as the still smaller waves of Hertz produce sparks in his little resonators, or, once more, as the waves emitted by electrostatically charged vibrating atoms excite corresponding vibrations in our retina. It may be worth while to suspend at Kew a compass-needle with a natural period of swing of 6·6 seconds, and see whether it resounds to solar impulses. Another, but almost microscopic, recording needle with a period of $\frac{1}{17}$ second might also be suspended.

The charge on the oscillator used in the present set of experiments vibrates 300 million times a second, which, though slower than the electric quiverings on, say, a three-inch ball, is yet quick enough to demand care and attention.

With very large oscillators, such as that described at the beginning of this paper, no such minute precautions need be taken.

* *Cf.* Mr. Oliver Heaviside, Phil. Mag. February, 1888, p. 152.

Fig. 4.—Small Oscillator used for optical experiments. Scale $\frac{1}{8}$.



Plates 8 centim. diameter.
Knobs 2 centim. diameter.
Each rod 6 centim. long and 1 centim. diameter.
Spark-gap about 8 millim.

Static capacity, $\frac{S}{K}=1{\cdot}4$ centim.

Self-induction, $\frac{L}{\mu}=190$ „

Characteristic factor, $\log\frac{4l}{d}=4{\cdot}5$.

Rate of vibration, 300 million per second.
Wave-length, 1 metre.
Dissipation-resistance, 7250 ohms.
Initial stock of energy, about 5400 ergs.
Power of initial radiation, 128 horse-power.
Number of vibrations before energy would be *at this rate* dissipated, about $1\frac{1}{2}$.

My oscillator is a good deal dumpier, and its ends have more capacity, than those of corresponding wave-length used by Hertz; the reason being that I prefer to make the electrostatic capacity bear a fair relation to the electromagnetic inertia, so as to gain a reasonable supply of initial energy for radiation. The store of energy is proportional to the capacity; the rate at which it is radiated per second is independent of it. Large terminal capacity helps to preserve a high potential longer, and so prolongs the duration of the discharge.

The wave-length of the emitted radiation is easily calculated approximately from the expression

$$\lambda=2\pi\sqrt{\left(\frac{L}{\mu}\cdot\frac{S}{K}\right)},$$

where $\frac{L}{\mu}=2l\log\frac{4l}{d}$; l being the length of the entire rod portion of the oscillator, and d its diameter*. The measurement of l is the most unsatisfactory part. It is best to

* See Addendum at end.

include the knobs and spark-gap as part of the whole length; the constriction at the spark will increase that part of the self-induction, but the expanse of the knobs will diminish another part. A trifle extra length should be allowed for the currents in the disks or balls at the end; but to measure l from centre to centre is rather too much allowance. From centre of one to nearest point of the other is a fair compromise.

As to S, it will be practically half* the static capacity of the sphere or plate at either end of the oscillator, especially if these are pretty big compared with the size of the rod. Strictly speaking they are not isolated, even when far from other conductors, because they are in presence of each other, but the correction is usually small. For instance, for two oppositely charged spheres of radius r, at a considerable distance l from centre to centre, the capacity is about

$$\frac{\frac{1}{2}lr}{l-r} = \frac{1}{2}r\left(1+\frac{r}{l}\right).$$

Hence the ordinary value of the capacity, as recorded for convenience below, is always a minimum which circumstances may increase but hardly diminish.

Values of $\frac{S}{K}$ *for Isolated Bodies.*

For a globe,	its radius.
For a thin circular disk,	$\frac{2}{\pi}$ times its radius.
For a thin square disk,	1·13 times inscribed circular disk, or ·36 times a side of the square.
For a thin oblong disk,	a trifle greater than a square of the same area.

Intensity of the Radiation.—Hertz has shown† that the amount of energy lost per half swing, by a radiator of length l charged with quantities $+Q$ and $-Q$ at its ends respectively, is

$$\frac{\pi^4 Q^2 l^2}{3K(\frac{1}{2}\lambda)^3}.$$

He omits the dielectric constant K, because he supposes Q

* *Half*, because the two spheres are technically "in series." See Addendum at end of paper.

† Wied. *Ann.* January 1889; or Nature, vol. xxxix. p. 452.

expressed in electrostatic units, but it is better to make expressions independent of arbitrary conventions.

So the loss of energy per second, being $\frac{v}{\frac{1}{2}\lambda}$ times the above, is

$$H = \frac{16\pi^4 (Ql)^2 v}{3K\lambda^4};$$

and this therefore is the radiation power.

For a given electric moment, Ql, the radiation intensity varies therefore as the fourth power of the frequency, *i. e.* inversely as the fourth power of the linear dimensions of the oscillator, as Fitzgerald some time ago pointed out.

But inasmuch as different oscillators will not naturally be charged to the same electric moment, but will rather be charged to something like the same initial difference of potential, as fixed by the sparking interval between their knobs, it will be better to write $Q = SV$, and to insert the full expression for λ.

Doing so, we get for the radiation activity at any instant when the maximum difference of potentials at the terminals is V,

$$H = \frac{\pi^4 S^2 V^2 l^2 v}{3\pi^4 K S^2 L^2 v^4} = \frac{V^2}{3K\mu^2 v^3 \left(2 \log \frac{4l}{d}\right)^2}$$

$$= \frac{V^2 K v}{12 \left(\log \frac{4l}{d}\right)^2} = \frac{V^2}{12 \mu v \left(\log \frac{4l}{d}\right)^2};$$

an expression roughly almost independent of the size of the oscillator. Quite independent of it if the length and thickness of its rod portion are increased proportionately.

(The factor μv may always be interpreted as 30 ohms whenever convenient.)

Thus all oscillators, large and small, started at the same potential, radiate energy at approximately the same rate; short stout ones a little the fastest.

But the initial energy of small oscillators being small, of course a much greater proportional effect is produced in them, and the radiation ceases almost instantaneously, their energy being dissipated in a very few vibrations. On the

other hand, oscillators of considerable capacity keep on much longer; and with very large ends, as in Leyden jars, the loss of energy by radiation is often but a small fraction of that turned into heat by the frictional resistance of the circuit.

The expression for the radiating power may be compared either with the form $\frac{1}{2}SV^2$ or with the form $\frac{V^2}{R}$; and the loss of energy may be said to be like a static capacity of

$$\frac{30 \text{ earth quadrants}}{6\left(\log \frac{4l}{d}\right)^2}, \text{ or } \frac{5556 \text{ microfarads}}{\left(\log \frac{4l}{d}\right)^2},$$

charged to the potential V, being discharged once a second; or like the heat produced per second in a resistance of $360\left(\log \frac{4l}{d}\right)^2$ ohms, having a difference of potential V between its ends. The duration of the discharge must therefore be exactly comparable to the time a wire of this resistance would take to equalize the potential of the oscillator-ends initially charged to the same difference of potential.

For the small oscillator used in the optical experiments here recorded, the value of $\log \frac{4l}{d}$ is approximately $4\frac{1}{2}$; hence the equivalent resistance is 7250 ohms. And, since the initial difference of potential is, say, 26,400 volts, the power of the initial radiation is 96,000 watts or 128 horse-power.

At this rate the whole original stock of energy (5400 ergs) would be gone in the two-hundred millionth of a second, *i. e.* in the time of $1\frac{1}{2}$ vibration; but of course the energy really decays logarithmically. The difference of potential at any instant being given by

$$\frac{d(\frac{1}{2}SV^2)}{dt} = \frac{V^2}{R}, \quad \text{that is, } V = V_0 e^{-\frac{t}{RS}};$$

where R is the above 7250 ohms plus the resistance of the spark and of the oscillator itself to these currents. The resistance of the spark is probably but a dozen, or perhaps a hundred, ohms; that of the small oscillator is about $\sqrt{(lr)}$ ohms, where r is its ordinary resistance to steady currents expressed in ohms, and l is its length in centimetres. This, therefore, is utterly negligible; practically the whole of its

energy goes in radiation. For the big oscillator the resistance is about $\sqrt{(\frac{1}{30}lr)}$; and so for a linear oscillator in general the dissipation resistance may be considered as simply

$$R = 360\left(\log\frac{4l}{d}\right)^2 \text{ ohms.}$$

Nothing approaching continuous radiation can be maintained at this enormous intensity without the expenditure of great power, a hundred and thirty horse-power if my calculation is right. Under ordinary circumstances of excitation the intervals of darkness are enormous; if they could be dispensed with, some singular effects must occur. To try and make the radiation more continuous a large induction-coil excited by an alternating machine of very high frequency, or by a shrill spring-break, might be tried. But even if sparks were made to succeed one another at the rate of 1000 per second, the effect of each would have died out long before the next one came. It would be something like plucking a wooden spring which, after making 3 or 4 vibrations, should come to rest in about two seconds, and repeating the operation of plucking regularly once every two days.

Statement of Results by Dr. HOWARD.

The apparatus used consisted of (1) an oscillator, or transmitter, with exciting coil; (2) a resonator or receiver, and (3) two lenses of pitch. We shall describe these in order.

The Oscillator or *Source of Radiation.*—This was made in two similar halves, each constructed by soldering to one end of a brass rod, 6 centim. long and ·95 centim. diameter, a thin circular copper disk of 4 centim. radius. To the other end was soldered a spherical brass knob of 1 centim. radius, highly polished. A small hole was drilled in each rod at a distance of 1·3 centim. from the knob to allow of the insertion of connecting wires to the Ruhmkorff coil by which it was excited. The two disks were cemented to two small wooden blocks which could be clamped in any position on a vertical glass rod. By this means the distance between the knobs could be easily adjusted, and the apparatus could be inclined when wished.

The induction-coil was of the usual pattern with hammer-

break. With the current used (supplied by 6 accumulator-cells) it gave a continuous stream of sparks between two points 2·5 centim. apart connected to the secondary terminals. The knobs of the oscillator were usually separated by a space of from ·7 to 1·0 centim. They required cleaning about every 20 minutes owing to burning produced by the spark. This burning was always greater at one knob than the other; greatest apparently at the one that mattered least, for if the primary current was reversed after the oscillator had been working some time the intensity of its radiations immediately decreased perceptibly.

The length of a *complete* wave emitted by the oscillator, calculated from its dimensions after the manner of Hertz, is 100 centim. And this is a sufficient amount longer than the conductor itself for the calculation to be not very inexact. It cannot pretend to accuracy.

The Resonator, or receiver and detector of radiation (the electric eye, as Sir W. Thomson calls it), was of the simplest possible construction. Two pieces of copper wire (No. 13 B. W. G.) were cut each to a length of 25 centim. One end of each was rounded off, and to the other end was attached a small rectangular brass scrap or plate at right angles to the wire. These little plates each carried a point; one of these points was fixed, and the other adjustable by a screw, by means of which the distance between them could be varied. The resonator was fastened to a piece of wood a little longer than itself. Its total length, including points and strips, was 53 centim., *i. e.* about half the calculated wave-length of the oscillator. A better mode of expressing it is to say that each half of the resonator is approximately a quarter wave-length, and corresponds to a closed organ-pipe, or to a resonant column of air in a glass jar.

The lenses were made of common mineral pitch, which was found to insulate quite well enough for the purpose. They were cast in the form of hyperbolic cylinders, bounded by a plane perpendicular to the axes of the principal hyperbolic sections; the eccentricity of the latter was equal to 1·7, and was taken as a fair approximation to the refractive index of pitch for infinitely long waves. A lens of this form should converge a bundle of parallel rays falling normally on its

plane surface to a line of foci coinciding with the outer foci of its principal hyperbolic sections ; and, *vice versâ*, rays proceeding from this focal line and falling on the curved surface should emerge from the lens as a bundle of parallel rays. Hence, if the oscillator be placed along the focal line of one lens, the electric rays from it will be sensibly parallel after traversing the lens, and after falling normally on the plane surface of the second lens should converge and meet at its focal line. The lenses were almost equal in size. Their plane surfaces were nearly square, being 85 centim. high and about 90 centim. broad. The greatest thickness (from vertex of hyperbola to plane surface of lens) is 21 centim. The lenses are each separated into an upper and lower half by means of a thin wooden partition inserted during the casting. It was intended to divide this partition by a saw-cut, and thus allow the lenses (each of which weighs more than 3 cwt.) to be more easily carried about. So far, however, this has not been done.

In making the experiments the lenses were placed one at each end of a wooden table $2\frac{1}{2}$ metres long, with their plane surfaces turned towards each other, and as nearly as possible parallel. The distance between them was 180 centim., and remained the same throughout the experiments. On one side of the table close to the edges of the lenses was a brick wall about 40 centim. thick ; and on the other side was a residue of gangway 54 centim. wide between the lens and a laboratory-apparatus cupboard, which has had to be set up in the corridor for want of space elsewhere. The oscillator stood, together with its exciting coil, on a small table whose height was adjustable; the plane of its disks was parallel to the flat surfaces of the lenses in all cases. It was intended to be placed in the focal line of the first lens ; but apparently the index of refraction had been assumed too high, and a position 51 centim. from the vertex of the lens seemed to do best. We shall speak of the vertical plane through the focal lines of the two lenses as the "*axial plane*." It contains the axes of the lenses and of the oscillator. Waves seem to be emitted more powerfully in this plane normal to the disks of the oscillator than in the plane containing them.

The direct effect from the oscillator could be perceived by

the resonator at a distance of 120 centim. in the axial plane in the most favourable case; that is to say, in a very dark room and just after cleaning the knobs of the oscillator. Under similar circumstances resonance was only just obtainable at the vertical edge of the first lens, viz. 85 centim. from the oscillator in a direction making an angle of about 30° with the axial plane. To get a rough measure of the intensity of the radiation at any point, the resonator was placed there, and its spark-gap arranged so as to just give a continuous stream of sparks; it was then brought to the line joining the oscillator and the edge of the first lens (line of reference), and the distance from the oscillator observed at which the sparks ceased to be continuous. When the intensity of the radiation was very small, however, the converse of this method was adopted; the resonator was adjusted at the line of reference and then taken to the point at which the intensity was to be observed.

The following are the phenomena observed in the space between the two lenses when the oscillator coincides with the focal line of the first one. The resonator gives brilliant sparks in the axial plane near the first lens so long as it is held parallel to the oscillator. On rotating it in a plane perpendicular to the axial plane the sparks decrease in brilliancy and length, and become entirely obliterated when the resonator and oscillator are at right angles. If the rotation is continued the sparks reappear and regain their former brilliancy, when the resonator again reaches its first position. If the resonator be placed in the axial plane and then moved parallel to itself towards the edge of the lens, the intensity of the sparking gradually decreases as we get nearer the edge, and on the side nearest the wall the sparks cease altogether at the edge of the lens. On the other side, however, they are visible right up to the edge of the lens, and then very abruptly cease, when the direct effect alone is obtained. The beginning of the sparking, as soon as the resonator enters the shadow of the lens, is very noticeable. The same appearances are observed at all distances from the first lens, but the intensity of the radiation is, of course, smaller as we get further from the oscillator. The radiations are always a little more feeble on the side nearest the wall than on the other side. The cause of this has not yet been definitely ascer-

tained, but it appears to be produced by some action of the wall itself. Slightly altering the position of the oscillator did not get rid of the effect; so it cannot be due to the oscillator being out of focus. There was apparently no defect in the lens itself which could account for it. The concentration of the radiations by the lens is very well marked. Just after passing through the first lens in the axial plane they are almost as intense as when they first impinge on its curved surface; that is to say, they do not lose appreciably in intensity by traversing the 21 centim. of pitch. But this concentration is even more clearly shown by the fact that in the axial plane, at the surface of the second lens (250 centim. from the oscillator), the sparks are quite as intense as the direct effect would be at 100 centim. in the same plane if the first lens were removed; or, again, the resonator will give sparks easily at the surface of the second lens, and when brought to the line of reference will not give sparks at a greater distance than 70 centim. At the surface of the second lens the irregularity mentioned above is a little greater than at the surface of the first one.

Beyond the second lens the rays are converged, as we expected, and there is a fairly well defined point in which they meet; but the intensity of the sparking at the focus of the second lens is not appreciably greater than at its surface. Probably this is due to the fact that the rays from the edge of the lens, having travelled a much longer distance in air than those in the axial plane, have thereby lost much of their intensity; and the differences between the intensities at different points could only be detected by a resonator with more delicate adjustments. The cone of rays between the second lens and its focal line is of almost uniform intensity in the neighbourhood of the axial plane. At the edge of the cone the intensity falls off very rapidly; and if the resonator be moved parallel to itself in a plane perpendicular to the axial plane, it shows sharply, by the commencement and stoppage of its sparking, where the boundaries of the cone lie. The cone is a little unsymmetrical on account of disturbance at the side nearest the wall, but the convergence of the rays to a focus is placed beyond a doubt.

The following observations were made on the rays after

they had passed the focus of the second lens. The resonator, after having been set to spark at a distance of 80 centim. from the oscillator in the line of reference, was taken to the focus of the second lens, and there gave sparks of fair intensity. Beyond the focus there were traces of a divergence of the cone of rays, which became more evident when the oscillator knobs were quite clean ; but in order to make sure of the existence of this divergence a more sensitive resonator would be necessary. In the axial plane itself the resonator used by us gave an effect when the conditions were most favourable, at a distance of 120 centim. beyond the focus of the second lens ; and it would possibly have given an effect still further away, had there not been an iron hot-water pipe 9 centim. in diameter running from floor to ceiling of the passage near this point. The furthest point at which any traces of sparking could be found was in one case 450 centim. from the oscillator, while without the lenses it was only 120 centim. This statement has to be taken along with the fact that the lenses were only 180 centim. apart, and that no attempt was made to elongate the parallel portion of the beam by increasing their distance.

In order to determine experimentally the wave-length of the oscillations, a sheet of tin-plate was set up against the flat (inner) surface of the second lens. The rays reflected from this plate were thus made to interfere with those incident on it so as to give stationary waves, as in some experiments of Hertz. The result was that close to the plate there were no traces of sparking. On taking the resonator further away, however, the sparks appeared, reached a maximum, and then disappeared again at a distance of 50 centim. from the plate. The point of disappearance was very definite. The sparks appeared again when the resonator was still further withdrawn, and as long as it was kept parallel to the oscillator no further disappearance of the sparks could be observed. By rotating it, however, in the axial plane, a point was found at which the amount of rotation required to make the sparks disappear was a minimum. In this position the centre of the resonator was 101 centim. from the reflecting plate. The observations agree with the previously calculated value of the wave-length, viz. 100 centim.

In the above experiments the oscillator was always placed in the focal line of the first lens, that is, vertically. Some observations were made later, after turning the oscillator through an angle so as to leave its centre in the axial plane, but its direction inclined to this plane. The effects were always of the same nature as those already described, even when both oscillator and receiver had been turned through a right angle, but the intensity of the radiation was not so great beyond the first lens. The focussing of the rays by the second lens could not be observed in this case, even when they were rotated only ten or twenty degrees, as the intensity was too small.

The above results all go to confirm the identity of electrical radiation and light; and are merely a slight extension of the famous researches of Hertz.

University College, Liverpool,
May 1889.

ADDENDUM *dated June* 20.

An expression for the self-induction of a straight copper rod, of length l and thickness d, we do not see how to calculate on Maxwellian principles without some sort of a return circuit somewhere. On action-at-a-distance principles it can be done thus:—

Consider two parallel filaments or thin straight wires at a distance c apart; call an element of one, at a distance a from some plane of reference, da, and an element of the other, at a distance b from the same plane, db. The mutual induction or potential of two elements on each other is

$$\frac{da\,db\cos\epsilon}{r},$$

where ϵ is the angle, and r the distance, between them. Hence the mutual induction of the two parallel filaments, each of length l, is

$$\mathrm{M}=\int_0^l\int_0^l \frac{da\,db}{\sqrt{\{(a-b)^2+c^2\}}}.$$

Integrating with regard to a, this becomes

$$M = \int_0^l \log \frac{\sqrt{\{(l-b)^2+c^2\}}+(l-b)}{\sqrt{(b^2+c^2)}-b} \cdot db,$$

where numerator and denominator are of the same form if the limits for the numerator portion have l subtracted from them both. Performing the integration of the two parts separately, and simplifying, we get

$$M = 2l \log \frac{\sqrt{(l^2+c^2)}+l}{c} - 2\{\sqrt{(l^2+c^2)}-c\}.$$

It may be worth while to write down the form this assumes when c is moderately small compared with l, viz.

$$2l\left\{\log\left(\frac{2l}{c}+\frac{c}{2l}\right)-\left(1-\frac{c}{l}+\frac{c^2}{2l^2}\right)\right\}.$$

If we now put for c the geometric mean distance of the points in a cross section of a rod of thickness d, we shall have the mutual induction of the parts of all the filaments in that rod upon each other, *i. e.* the self-induction coefficient of the rod. And unless the rod is very short and thick, it will be permissible to neglect the c/l terms.

Now the geometric mean distance of the points in a circular section varies from $\frac{1}{2}d$, when they are concentrated into its circumference, to $\frac{1}{2}e^{-\frac{1}{4}}d$, or $\cdot 3894\,d$, when they are spread uniformly all over it. The first case corresponds to our rapidly periodic currents, and gives, as the self-induction of a rod in which currents keep to the periphery,

$$L = 2l\left(\log\frac{4l}{d}-1\right);$$

whereas if the currents penetrate all through its section, by reason of being of slowly changing strength,

$$L = 2l\left(\log\frac{4l}{d}-\tfrac{3}{4}\right).$$

The difference is not marked : at least for the case supposed, of non-magnetic material.

Hertz employed this last formula, quoting it apparently from Neumann ; but he says that in Maxwell's theory the $\frac{3}{4}$ turns into $\frac{1}{4}$. We do not know how he makes this out, but suppose he is somehow right ; and it is this uncertainty which

has caused us to refrain from going into minutiæ on the subject, and to be satisfied with using merely $\log \frac{4l}{d}$, instead of $\left(\log \frac{4l}{d} - \text{something}\right)$, for what we have called the characteristic factor. It is easy to subtract 1 from it if that is the proper thing to do, as our calculation indicates it is. But the violent constriction at the spark, in the case of an oscillator, must cause a considerable increase of self-induction.

It may be interesting just to quote in similar form the self-induction of the same rod bent into a circle, viz.

$$2l\left(\log \frac{4l}{d} - 2 - \log \frac{\pi}{2}\right),$$

if the currents keep to its periphery. When they penetrate its section uniformly the 4 becomes 5·14, and that is all the change unless it is made of magnetic material.

It thus approaches the same value as the straight rod for infinite length, but is always distinctly less.

There is one point on which we find ourselves differing from Hertz. We regret to say that our calculation of radiation-intensity comes out four times as great as his. We get the same formula as he does, so there is no slip in the working there; but, in the application, a 2 or a $\sqrt{2}$ comes in wrongly in one or other of our calculations. His using half-wave lengths is a natural source of confusion, but we have avoided all that; and it must be that it is owing to a different calculation of the effective capacity concerned in an oscillator that the discrepancy arises. If an oscillator has spheres 30 centim. diameter at either end, Hertz calls its capacity 15 centim.; we call it $7\frac{1}{2}$. We feel bound to call it $7\frac{1}{2}$ according to any method of calculation; although the radius of either sphere is the natural thing to write down at first thought. The charge which surges into either sphere has had to come from the other, not from the earth or anything of infinite capacity. The two spheres are therefore like two condensers in series. Hence our wave-lengths are $1/\sqrt{2}$ of Hertz's wave-lengths (or rather $\sqrt{2}$ times what he *calls* his wave-length); and since λ occurs to the fourth power in radiation intensity, it makes our radiation 4 times as strong

for a given oscillator as that which he would calculate. This discrepancy we by no means view lightly, and it is not without many qualms that we find ourselves differing, even about a 2, with a man so splendidly careful in his work as Hertz has shown himself. It is more than probable that he is right after all, so we explain what will then turn out to be our error in this note.

Prof. Fitzgerald congratulated the authors on their success, and also pointed out that, although large oscillators give good results at distances within a few wave-lengths, yet at greater distances small ones were decidedly superior, owing to the energy of radiation varying as the 4th power of the rapidity. He had recently made experiments on electric radiations analogous to Newton's Rings, and had successfully observed the central dark spot and the first dark band. Referring to Dr. Lodge's experiments, he inquired whether any traces of diffraction were observed near the boundary of the bundle of rays between the lenses. Speaking of polarization-experiments, Prof. Fitzgerald said waves reflected from films of water exhibited no polarization, whereas those reflected from nonconductors were completely polarized.

In reply Dr. Lodge said no diffraction-effects had been observed, but in the interference-experiments to determine wave-length the positions of minimum effect were very decided.

XXIV. *An Apparatus Illustrating Crystal Forms.* *By* R. J. Anderson, *M.A., M.D., Professor of Natural History in Queen's College, Galway**.

[Plate III.]

The apparatus by which I propose to illustrate crystal forms consist of frameworks and cords and weights.

The first piece of apparatus is figured in Plate III. fig. 1, and consists of a frame made of wood. This is divided into two compartments. One of these has, above, a slit half an inch wide that runs from end to end; in this slit a slide moves to and fro, and can be fixed by means of a binding-screw at any place.

A slide of a similar kind moves in a slit in the lower part of

* Read April 13, 1889.

the framework; this can be fixed by binding-screws in any position. Pulleys are fixed at the ends of this compartment.

Slips of wood run from end to end at the sides and carry riding-slides. These slides have binding-screws and pulleys whose sheaves revolve on a vertical axis fitted to them.

A figure is easily constructed by carrying cords over the pulleys. Single cords only are shown in the figure. This is for the sake of distinctness.

Starting from α', which marks a ring connected with the weight p, a single cord runs through β' (ring), η' (ring), δ', through ring α', through η', to be fixed to a weight.

A second cord starting from γ' runs through α', ϵ', η', through ring γ', and then across to ϵ' through this ring to hook up a weight p'. A third cord is fixed to δ' and runs through ϵ', β', γ', through δ' and β' to loop up another weight. The actual tension-weights are fixed to the small rings, which act as pulleys.

Each rhombus has in this way a cord to itself, and the size of the rhombus depends on the weights attached. The smaller the rhombus the more cord is to spare.

The figure shown is the regular octahedron if the axes be equal. This condition is easily produced by adjusting the weights.

The octahedron of the second dimetric system, or pyramidal system, is produced by increasing the weights above and below.

The octahedron of the third system may be easily formed by increasing a pair of the horizontal weights.

The octahedral figures may be easily formed by leaving out the diagonals and running the cords from the rings at one extremity of the rhombuses through two rings, and then through the opposite ring, to be there fixed to a weight. The tension-weights, as shown in the figure, will then correspond to the apices of the rhombuses.

For the oblique systems further changes are necessary. The upper slide is moved to the right and the lower to the left, or *vice versâ*. This is attended with elongation of the vertical axes, and the cords passing through the pulleys above and below and at the ends are increased, and the slack below is pulled in to a less extent. The other sides of the octahedron are less affected.

In the first place, the lateral rider-slides are allowed to

remain in a position such that the line joining them is perpendicular to the central vertical longitudinal plane. This gives the Monoclinic System.

Secondly, the rider-slides are moved one to the right, the other to the left, and in this way the *Anorthic* or Triclinic System is produced.

In each case it is desirable to have the slack for each rhombus at different angles of the octahedron.

All the possible varieties of the fifth system cannot be produced in this way. So it is necessary to arrange for the elevation and depression to the rider-slides in extreme cases. This is accomplished by means of a large ring which carries a pulley.

I have chosen the octahedron as the simplest figure.

The cube is formed by the introduction of two horizontal hoops, one above and one below the level of the horizontal bars. These by a simple mechanism are made movable; and if eight pulleys be fixed opposite the eight edges of the octahedron, and the edges of the octahedron be drawn out by rings running on these cords, it will be necessary, then, only to run cords through rings above and below, and to relax the horizontal and apical weights in order to produce the cube.

The modifications caused by truncating or bevelling the edges or faces can be produced by increasing the number of the hoops or rings. For the simplest figures, however, vertical hoops answer best. The sliding-rings that are carried by the cords may with their transverse cords be lowered to the level of the bar again, and the octahedron again produced.

The cube and the corresponding forms of the pyramidal and prismatic systems may be easily constructed by running the cords as follows:—

Take the cube as

$$\begin{matrix} a' & b' & c' & d' \\ e' & f' & g' & h' \end{matrix}$$

where the first row represents the upper face, and the second the lower, as in fig. 2.

The cords will have the following course:—

$$\begin{matrix} b' & g' & h' & f' & b' \\ e' & d' & c' & f' & e' \\ f' & a' & d' & g' & f' \\ c' & h' & e' & b' & c' \end{matrix} \qquad \begin{matrix} a' & b' & g' & h' & a' \\ b' & c' & h' & e' & b' \\ c' & d' & e' & f' & c' \\ h' & d' & b' & f' & h' \\ g' & c' & a' & e' & g' \end{matrix}$$

The faces of the cube corresponding to the angle of the octahedron.

Now by drawing out the cords opposite the middle of the faces (that is, the diagonals of the faces) a 24-sided figure is produced which can be reduced in the limit to a 12-sided figure, namely the rhombic dodecahedron.

If the sides be connected by cords with pulleys and drawn out, and at the same time cords connecting the centres of the sides with the centres of the faces be drawn out, then the trapezohedron is produced.

Cause the two lateral pulleys of the cube to approach above and the longitudinal pair to approach below, and the tetrahedron is produced. A prism surmounted by pyramids is produced by drawing out the diagonals of the terminal faces; from this the corresponding octahedron may be obtained by a simple method.

The other part of the framework is shown in the drawing (fig. 1, left-hand) as containing the double hexagonal pyramid. Sliding-pulleys, as in the part already described, are fitted above and below. Rider-pulleys are attached to the bars at the sides. Two are shown on each side.

The cords are attached in this way :—A bundle of six are fastened together above to a cord, and drawn by this cord through a ring. The cord passes over two pulleys and reaches a weight outside the framework. The six cords pass through the rings marked β, γ, δ, ζ, θ, and ϵ in the figure, then down to be attached to a cord below, which goes through a pulley-sheaf. The rings marked by the Greek letters are seen attached by cords to weights, through each of these a cord passes. This cord is carried through one of the rings and kept there by means of a small weight. The ratio of the vertical to the horizontal axes may be easily altered by means of the weights. The approximation of the lateral pulley gives rise to the octahedron. The number of the sides may be increased by increasing the cords and pulleys.

In order to show other figures two hoops are fitted to the framework, above and below. The cords of the pyramids are hooked out, and the cords connected with the hooks pass over pulleys and are attached to weights. A cord is made to go through the rings (hook-rings) above and below.

By running down the rings and unhooking the weights above and below, the hexagonal prism is produced.

Prisms with more sides can be produced by increasing the number of the cords which correspond to the edges. The pyramids surmounting the prism are produced by drawing out the cords at the extremities of the prism. Figures with fewer sides are produced by causing the pulleys to approach. Forms the result of bevelling and truncation are produced by pulling out the cords of the terminal pyramids and running other cords through the rings. The original double six-sided prisms are produced by causing the hoops to approach one another.

The ikosahedron is produced by forming the five-faced equilateral pyramid above and below, and approaching the hoops towards one another, so that the distance between the hoops is equal to the perpendicular of one of the triangles. Then it is only necessary to rotate the lower hoop though 36°, and to connect the obtuse angles of the rhombus. In this way the figure can be produced.

The relations of the hexagonal to the rhombohedral division of the sixth system may be shown in this way. Take the double pyramid, hook up each alternate horizontal angle, and hook down the others. Adjust suitably the superior and inferior angles, and the rhombohedron is produced. The cords in reality follow the course of the lines in the glass models.

This method is very interesting in this way, that by a little dodging the rhombohedron can be converted into the cube, so that the relations between the sixth and other systems are rendered more distinctly apparent.

The rhombohedron may be easily changed into the hexahedron by unhooking the weights and pulling in the cord. The hoops are shown in the lower part of fig. 3, Pl. III., with the rhombohedron attached. The hexagonal prism is figured separately for the sake of distinctness.

The ikosahedron may be produced by hooking up and down the horizontal cords of the decahedral pyramids. If we begin with the double octahedral pyramids, the rhombic dodecahedron can be easily produced by hooking up the corresponding alternate edges above and below, and running cords through the hooks looped up and those rings still stationary.

In order to show the effects of uniting and separating forces the form shown at fig. 2 is useful. The instrument consists of a frame in which hoops revolve, some on vertical and others on horizontal axes. The hoops carry sliding-pulleys as shown on the plate. The cube is easily constructed by running cords over eight pulleys fixed on two rings revolving on a vertical axis. Cords are carried through small rings above and below (fig. 2, $a' b' c' d' e' f' g' h'$).

Without going into details, it will be easily seen that one orthogonal hexahedron can be easily changed into another, and into the corresponding octahedron. The octahedron of the first system, $a b c d e f$, if constructed by running cord over the pulley B, and the pulley attached to the same ring below, may be changed into the octahedron of the dimetric or trimetric system, or of either of the oblique. The latter is accomplished by causing the hoop to revolve, and for the triclinic the vertical hoops come into action. Adjustment of the weights leads to an alteration in the axes, and the relations of the weights for a special form may be studied.

It is evident that the dodecahedron and trapezohedron may be produced in this instrument as in the first, and that the forms due to truncating or bevelling of the sides are obtained very readily.

The following are the advantages of the apparatus:—First, it shows clearly the effect of changes of force in producing changes of form. The weights can be approximated or separated, and thus the relations of allied forms may be studied. The number of weights may be increased, and the change of form by grouping may in this way be well shown.

If we take an india-rubber tissue ball inflated with air as an example of an infinite number of forces acting from a centre, and a piece of stretched cord with a weight attached as the other extreme limit, many of the intermediate conditions where strings are made to form the edges of figures may be easily understood from the arrangements I have described.

It is true that such methods as are here suggested are open to the objection that mathematical principles of a very important kind are involved. I think the same objection may be made to any mechanical contrivance; but so far from getting rid of a difficulty without explaining it, I hold that

the apparatus, whilst it will produce a better conception of crystal forms, and the actual work in the crystals themselves, in the minds of those students who know very little about mathematics, viz.. almost all students of chemistry and mineralogy, and a still larger number of geological students, the apparatus will prove useful to mathematical students inasmuch as the arithmetical processes are tedious and complex for even those forms in which the mathematical relations are comparatively simple. For the forms with oblique axes the advantages of a simple method of noting the weights necessary to maintain equilibrium far outweigh the disadvantages.

Note.—Professor Wiltshire informs me that many years ago, Mr. Mitchell, at the Royal Institution, showed a model by which the derivation of the crystalline systems from the octahedron was explained.

EXPLANATION OF PLATE III.

Fig. 1.—$\alpha', \beta', \gamma', \delta', \epsilon', \eta'$, octahedron.
p, p', weights.
$\alpha, \beta, \gamma, \delta, \epsilon, \zeta, \eta, \theta$, double hexagonal pyramid.

Fig. 2.—a, b, c, d, e, f, octahedron.
$a', b', c', d', e', f', g', h'$, cube.
A, B, C, D, E, F, weights.

Fig. 3.—The upper figure shows the hexagonal prism surmounted by hexagonal pyramids. The lower shows the rings with the rhombohedron formed.

XXV. *A Shunt-Transformer.* *By* Mr. E. W. SMITH*.

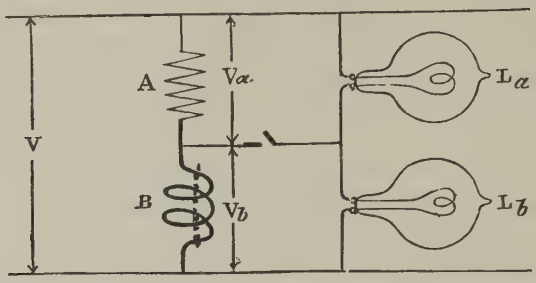
ALTHOUGH this experiment has already been described by Professors Ayrton and Perry in a paper at the Institution of Electrical Engineers, it was thought to be worth while occupying this Society's time in showing it here, as it forms a good lecture-experiment, if nothing more, to illustrate acceleration and lag of alternating currents.

The experiment consists as follows:—Between two leads a certain alternating potential difference, V, is maintained. We have two resistances, A and B (fig. 1), in series, through which part of the current flows. If the impedance of A is equal to that of B, then P.D. at terminals of A, V_a, and P.D.

* Read June 8, 1889.

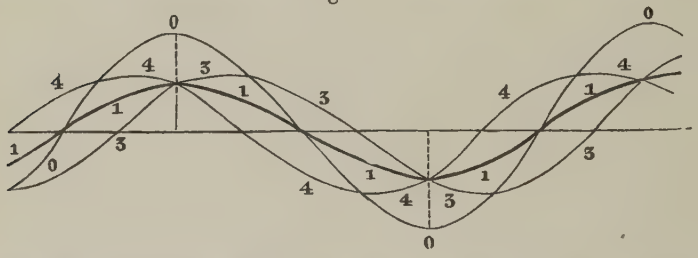
at terminals of B, V_b, are equal. If A and B are alike in resistance and self-induction, then V_a and V_b would be in the

Fig. 1.



same phase and each equal to $\frac{1}{2}V$. If curve 0 0 0 0 (fig. 2) represents V, then curve 1 1 1 1 represents $\frac{1}{2}V$. But if A

Fig. 2.



has large resistance and B much self-induction, then, although V_a may be still equal to V_b, V_a will be lagged and V_b accelerated. Then curve 3 3 3 3 will represent V_a, and 4 4 4 4 V_b.

We have also two similar sets of lamps, L_a and L_b (fig. 1) in series between the two main leads. Under ordinary circumstances they would each have a P.D. of $\frac{1}{2}V$ (curve 1 1 1 1, fig. 2) at their terminals. But if we connect the junctions of the two sets of lamps with the junction of the two resistances, then lamp L_a will have P.D. V_a, and lamp L_b a P.D. V_b; and therefore *both lamps will become brighter*. If the lamps take an appreciable current, then, when the junctions of the different circuits are joined, the current in the inductive resistance as well as in the lamps becomes greater, but that in the *main circuit becomes smaller*, as may be seen by a dynamometer.

The resistance A may of course consist of lamps, and B may be a choking coil, which absorbs very little energy.

Prof. Ayrton has given this inductive part of the circuit the name of a "Shunt-Transformer."

I have made a similar experiment with one of Mr. Mordey's transformers wound with three coils, each having the same number of turns. Using one of these as a primary and the other two as two independent secondaries, then, by having one circuit comparatively non-inductive and the other inductive, one gets the arithmetic sum of the amplitudes of the secondary currents *greater than that of the primary current*, although of course the vector sum must be less. For instance, take one particular experiment. The primary was on a circuit having a P.D. of 128 volts. One secondary, A, was composed of lamps, and the other, B, was a Tesla motor.

Volts in primary,	128	Current in primary,	14·2	
„ secondary A,	119	„ secondary A,	8·0	16·2
„ secondary B,	119·5	„ secondary B,	8·2	

Connecting up the secondaries of transformers A and B in parallel, the volts at the terminals of the primary being the same as before, and the secondary circuit being lamps.

Volts in primary,	128	Current in primary,	17·4
„ secondary,	121	„ secondary,	16·3

Here we have to give 17·4 amperes to the primary instead of 14·2 to get same current in secondary, and the volts in secondary are practically the same as before.

If we were being supplied with electricity, what should the meter measure? Surely the amount of energy we use. But ordinary meters only measure ampere-hours, and so cannot but give records in favour of consumer or supplier. The sooner the public understand this, the sooner we shall have a scientific meter in our houses.

XXVI. *On the Use of the Biquartz in determining the position of the Plane of Polarization.* *By* A. W. Ward*.

The biquartz has been so often used, especially on the Continent, by investigators on the rotation of the plane of polarization of light, and with such extremely varying

* Read June 8, 1889.

degrees of accuracy, that it seems of interest to account for these results mathematically. Verdet and H. Becquerel obtained results which varied by less than 4′; while Wertheim, Matteucci, Edlund, Lüdtge, and Villari obtained results varying by as much as 2°. Lüdtge has in one case obtained a rotation of 4° where, on his own showing, the light was circularly polarized. Verdet's and Becquerel's accurate results were obtained when rotation was looked for in liquids and isotropic substances; and the inaccurate results of Wertheim &c. were obtained when seeking for a rotation in doubly-refracting substances. In the former case the light remained plane-polarized, in the latter it became elliptically-polarized, and the position of the plane of polarization was really that of one of the axes of the ellipse. In the present investigation we shall then determine with what degree of accuracy the biquartz can be used to determine the position of the axes in elliptically-polarized light.

Let us suppose that the elements of the elliptically-polarized light are given by the displacements along the axes of the ellipse, and by the inclination of an axis of the ellipse to some direction fixed in space. Let the displacements ξ and η be parallel to the axes of the ellipse, and let the axes of x and y be fixed in space, z being the axis along which the light travels; and let ω be the angle between the axes of x and ξ. If then c^2 be the intensity of the light, $\tan\gamma$ the ratio of the axes of the ellipse, the vibrations of the light are given by the equations

$$\left.\begin{aligned}\xi &= c\cos\gamma\cos\frac{2\pi}{\lambda}(vt-z),\\ \eta &= c\sin\gamma\sin\frac{2\pi}{\lambda}(vt-z).\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (1)$$

The angles γ and ω are known whenever we know the history of the light; how it became converted from plane-polarized into elliptically-polarized. If, for instance, the change took place in passing through a doubly refractive medium whose axes are those of x and y, then

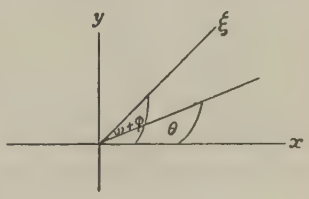
$$\left.\begin{aligned}\tan 2\omega &= \tan 2\alpha\cos\beta,\\ \sin 2\gamma &= \sin 2\alpha\sin\beta,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

where β is the total angular retardation, and α the inclination

of the initial plane of vibrations to that of xz. In these equations β is a function of λ, viz. $\frac{2\pi}{\lambda}(\mu_1-\mu_2)z$, where μ_1 and μ_2 are the indices of refraction along the axes of x and y respectively. If α is small, variations in ω due to λ are not important; but if α is large this is no longer the case, as we shall eventually see.

Let us now pass the light (1) through a biquartz which is such that the plane of polarization of light, of wave-length λ, is turned through an angle ϕ. This rotation will simply turn the ellipse as a whole, and not affect the ratio of the axes. Hence for upper half of the biquartz ω becomes $\omega+\phi$, and for lower half $\omega-\phi$.

Let the light be now analysed by a Nicol whose plane of vibrations makes an angle θ with the plane of xz. If then h^2 be intensity of light passing through the upper half of biquartz, and k^2 that of light passing through the lower half, we have, as usual,



$$\left.\begin{aligned} h^2 &= c^2\cos^2\gamma\cos^2\overline{\omega+\phi-\theta}+c^2\sin^2\gamma\sin^2\overline{\omega+\phi-\theta},\\ k^2 &= c^2\cos^2\gamma\cos^2\overline{\omega-\phi-\theta}+c^2\sin^2\gamma\sin^2\overline{\omega-\phi-\theta}; \end{aligned}\right\} \quad (3)$$

or

$$\left.\begin{aligned} h^2 &= \frac{c^2}{2}\{1+\cos 2\gamma\cos 2(\omega+\phi-\theta)\},\\ k^2 &= \frac{c^2}{2}\{1+\cos 2\gamma\cos 2(\omega-\phi-\theta)\}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

We have now to determine what value of θ makes $h^2=k^2$ *for all values of* λ.

Equating h^2 to k^2, we get

$$\cos 2\gamma\sin 2\phi\sin 2\overline{\omega-\theta}=0. \quad . \quad . \quad . \quad . \quad (5)$$

This equation is satisfied whenever

$$\cos 2\gamma=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or

$$\sin 2\phi=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

or

$$\sin 2(\omega-\theta)=0. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The first of these solutions occurs when $\gamma = \frac{\pi}{4}$, *i. e.* when the elliptically-polarized light is really circularly polarized. In this case the phrase plane of polarization has no meaning at all, and so it need no be discussed.

The second solution (7) gives $\phi = \frac{\pi}{2}$. This can only be the case for one particular wave-length, and depends simply on the thickness of the biquartz. A biquartz is usually made of such a thickness that ϕ is $\frac{\pi}{2}$ for the yellow light from the brightest part of the spectrum. We shall suppose this to be the case here.

The third solution gives

$$\omega = \theta.$$

If, then, this solution does not hold for all values of λ, then, however the analysing Nicol be turned, both halves of the biquartz can never be made of the same uniform tint.

Now, considering the equation

$$\tan 2\omega = \tan 2\alpha \cos \beta,$$

we see that $\omega = \alpha$ always if $\beta = 0$, that is, if the incident plane-polarized light always remains so. If β is not equal to 0, then still $\omega = \alpha$ for all values of λ, if $\alpha = 0$ or $\frac{\pi}{4}$. If α is 0, then $\gamma = 0$, and the light is plane-polarized as before. The case we have to discuss, then, is $\alpha = \frac{\pi}{4}$.

If $\alpha = \frac{\pi}{4}$, and also for any particular value of λ, $\beta = \frac{\pi}{4}$, then light of that colour is circularly-polarized. Hence, however we alter θ, no change in the intensity of that light will take place. If this circularly-polarized light comes from a prominent part of the spectrum, it will be impossible to note small change in the tint of passage due to the varying presence of other colours. The difficulty experienced will be precisely similar to that of fixing the position of the plane of polarization by means of the yellow tint of passage instead of the violet tint.

If β is never so great as $\frac{\pi}{2}$, then, when both halves of the biquartz are of the same uniform tint, the position of the ana-

lyser determines the position of the ellipse; but the uniform tint will not be that due to excluding the yellow light of the spectrum, but will contain lights of every colour, but not in that proportion which constitutes white light. The tint may be rosy or yellow.

If α is neither 0 nor $\frac{\pi}{4}$, then ω varies with λ, and cannot possibly satisfy the solution for all values of θ. In this case, then, both halves of the biquartz can never be made of the same tint. As this is the general case, we conclude that the biquartz is not a suitable instrument to use when, instead of plane-polarized light, we have elliptically-polarized.

The following table gives the values of ω due to variations in α and λ when the light has passed through a quarter undulation-plate of quartz. The values have been calculated from Rudberg's table of indices, quoted on p. 317 of Glazebrook's 'Optics.' The capital letters refer to the lines of the spectrum.

		C.	D.	E.	G.
	β.	80°.	90°.	100°.	126°.
α=10°.	ω	2°	0	−2°	−6°
α=20°.	ω	4°	0	−4°	−13°
α=44°.	ω	39°	0	−39°	−43°

The above is simply given as an illustration of the magnitude of the quantities involved in a particular case where it is easy to make the calculations. I have tried the experiment by passing light the reverse way through an elliptic analyser (*i. e.* a Nicol prism and quarter undulation-plate), then through the biquartz, and finally through an analyser. It is found quite impossible to get any match between the two halves of the biquartz when α is large. The actual dispersion of the axes depends upon the variations of β with λ, and this is very much greater in quartz than in such a doubly refracting substance as compressed glass. But in most cases there will always be sufficient variation to make the use of the biquartz a very unsuitable method, and this does, I think, account for the two classes of results mentioned at the beginning of this paper.

In conclusion, I have only to express my gratitude to Mr. Glazebrook for many valuable suggestions, and to Professor J. J. Thomson for the use of the Cavendish Laboratory.

XXVII. *Intermittent Lightning-Flashes.* *By* H. H. Hoffert, *D.Sc., A.R.S.M., Demonstrator of Physics at the Normal School of Science and Royal School of Mines* *.

[Plate IV.]

The storm which passed over London on the evening of June 6th afforded an unusually favourable opportunity for observations, both with and without the aid of the camera, on the character of lightning-flashes, and for determining the causes of some of the curious effects noticed by previous observers.

While watching the storm from my house in Ealing I could in several instances distinctly perceive a flickering appearance in a discharge, and in one particular case the repetitions were at least 5 or 6 in number, just sufficiently slow for the eye to detect the variations in brightness without removing the impression of one single flash. Other observers with whom I have since spoken have informed me that they had observed a similar effect, and that in some instances flashes, following as nearly as could be seen in the same path, were separated by an appreciable interval, often of several seconds' duration. Photographs of lightning have frequently been obtained showing banded, ribbon, or double flashes; but, so far as I am aware, these have not been of so decided a nature as to remove doubts whether the appearances could not be ascribed to the effects of halation by reflexion from the back of the negative, or to blurring from the photograph having been taken through the glass of a window, or to some other similar cause. I was therefore anxious to obtain some decisive evidence of the repetition of the flashes, and as my friend Mr. G. J. Snelus was at my suggestion attempting to obtain some photographs of the lightning, I joined him, and he kindly placed his camera and some plates at my disposal.

The rain having ceased for the moment, I was able to go out on to a balcony and thus get a good view of the storm, which had now approached quite close and seemed to surround us. The camera was held in the hand and pointed towards

* Read June 8, 1889.

the north-east, where, about half a mile away, numerous brilliant flashes were occurring. The cap of the camera was taken off, and then the camera was moved in a horizontal plane about the lens as a centre at the rate of about once to and fro in three quarters of a second, until a flash was observed in the direction in which it was pointed, when the cap was at once replaced. The duration of the exposure of the plate was about half a minute. The camera was of quarter-plate size, the plates were Ilford rapid, and the lens, which was a rapid rectilinear, was used with full aperture (*f*/8).

I hoped, by having the camera moving, to be able to separate the successive components of the flashes, and in this I was fortunately successful. In fig. 1 (Pl. IV.) is a reproduction of one of the photographs obtained, and on it can be seen two triple flashes (I., II., III., and IV., V., VI.), and one double flash (VII. and VIII.).

During the interval that the plate was exposed the illumination of the sky from flashes out of the line of view, or behind clouds, produced the glare seen in the centre and upper part of the photograph, and some faint flashes which were noticed were probably the cause of the streaks seen in fig. 2 at *o*, *p*, and *r*.

A careful examination of the photograph reveals many interesting features. The three successive flashes I., II., III. are identical in form. If the negative be placed over a print so that either of the three lines on the negative lies over either of those on the print, the coincidence is seen to be exact even to the smallest irregularities. Nevertheless, of all the branch-flashes which spread out from I. only a small trace exists at *n* in II., and none whatever in III. Sweeping across the photograph and connecting corresponding points in the successive flashes are streaks of light, showing that a very considerable residual illumination remains between the discharges. These streaks are especially well marked between the components of the double flash VII., VIII., and are also very bright along the path of the head of flash I. They are not always present; for in another photograph obtained upon a moving plate by Mr. Snelus, in which a flash is reduplicated, there is no trace of them, the flashes being quite sharp and distinct. The streaks commence abruptly with one discharge

and end abruptly with another. Their extension to the left of I. was probably due to the camera not having quite reached the end of its swing. Where they are brightest there is in all cases a swelling at the part of the flash where they start.

At the upper left-hand corner there is a curious dark flash with bright edges and short luminous streaks. The form of this dark flash is exactly reproduced in the two bright flashes V. and VI. Dark flashes are frequently met with in photographs of lightning, and have been usually ascribed to reversal of the image by overexposure; but I do not think this explanation applies in the present case, both on account of the appearance which the dark flash presents, and also because as far as I can recollect the brilliant downward flash I. appeared to the eye much the brightest of those that occurred while the plate was exposed. There is a similar but less distinct dark gap to the left of VI.; and to the right of II. are three or four faint bands parallel to it and following its sinuosities. These all seem to be due to variations in brightness in the luminous streaks, which are thus shown to be electrical and not phosphorescent in character.

When carefully compared, the forms and positions of the flashes I. to VIII. are found to present such agreement as to show that they must have formed a system of discharges closely connected together. If V. be placed over IV. so as to coincide with its left-hand border, the portion *d* exactly coincides in form and position with the portion *a* of I., and both *a* and *d* terminate in a curious bifurcated enlargement which, when examined with a lens, shows a beaded appearance, roughly sketched in fig. 3. This coincidence of form and position would be difficult to account for if I. and IV. were independent flashes not occurring at the same time. It is evident, however, that the series I., II., III. cannot have occurred during the same sweep of the plate as IV., V., VI., since the curve joining *a*, *b*, *c* is concave upwards, while that joining *a*, *d*, *e* is concave downwards. The flashes VII., VIII. seem also to have formed part of the same system of discharges, for the part of VII. from *f* to just above *h* is coincident with the upper portion of I., while the luminous streaks extending between VII. and VIII. agree in their directions with the curves joining *a*, *b*, *c* and *a*, *d*, *e*, those at *g* being

parallel to the line joining *d* and *e*, and those at *k* following nearly but not exactly the line of *a*, *b*, *c*. There is thus afforded some clue to the determination of the order of the discharges, and I think the order was probably as follows:—VII., VIII., VI., V., IV., I., II., III., the first two occurring during one sweep of the camera; VI., V., IV. during the backward sweep; and I., II., III. in the next onward sweep. There must thus have been an interval of a little over a second between the first and last discharge, for the motion of the camera was at about the rate of three quarters of a second for a complete swing to and fro. The interval between the successive discharges was, therefore, between the fifth and tenth of a second. Had it been much longer the flash would have presented to the eye a flickering appearance.

Whatever be the explanation of some of the effects noticed above, it is evident that a lightning flash has not the simple instantaneous character formerly supposed; but that it consists of a varying number of successive discharges following one another in the same path at intervals which may in some cases be comparatively long.

Note.—Since the above was written I have had an opportunity of inspecting the photographs of banded, ribbon, and curtain lightning collected by the Royal Meteorological Society. There is, I think, no doubt that the explanation of these is afforded by the multiple flashes and luminous streaks noticed above. It is noteworthy that they were always obtained with a camera held either in the hand or in such a way as to render motion probable.

Prof. Herschel, who had taken photographs during the same storm, referred to the fluttering appearance of the flashes and on their long duration; in many cases the time was sufficient to allow him to direct the camera towards the flash and make a successful exposure. He had also observed multiple flashes with the unaided eye, and on waving his hand about he had sometimes noticed about a dozen distinct images of it during one discharge.

Mr. Gregory said that he watched the storm along with two others, and they could seldom agree as to the shape of the flashes or on their simple or multiple character. The want

of agreement as to multiplicity he thought might be caused by their eyes being directed towards different parts of the sky when a multiple flash occurred; the one who happened to be looking towards the flash might be conscious of only one impression, whereas the others in directing their eyes would receive the flashes on different parts of the retina. In some cases as many as three distinct flashes (occurring at intervals of about ten seconds) traversed the same path, and a number of the discharges presented a beaded or striated appearance. The beads seemed to remain after the main flash had faded, and this might account for the bands shown in Dr. Hoffert's photograph.

Mr. C. V. Boys, in referring to multiple flashes, said that although his statements made in the discussion of Mr. Whipple's paper on April 13th were not readily accepted, yet no one who watched the recent storm could doubt their existence.

Prof. S. P. Thompson thought the order of the flashes on the photograph may have been the reverse of that supposed; for he observed that the bands of light extended on both sides of the (so-called) primary flash, whereas the outside of the third flash was quite dark.

Mr. E. W. Smith noticed many cases of "sympathetic discharge" in which a flash in the north seemed to precipitate another in the north-west within a few seconds; and in this he was corroborated by Mr. Gregory, who viewed the storm from a different locality.

Mr. C. V. Burton thought the heating of the air by the first spark of a multiple flash might give rise to the tributaries to the succeeding sparks.

Mr. A. W. Ward mentioned a long flash observed at Cambridge which passed from the zenith and struck some farm-buildings at a distance, and he was particularly impressed by the considerable time occupied in its progress.

XXVIII. *Note on some Photographs of Lightning and of "Black" Electric Sparks.* *By* A. W. CLAYDEN*.

DURING the thunderstorm on the night of June 6 I exposed several plates in the hope of securing photographs of lightning. Three of these gave results.

* Read June 22, 1889.

One was exposed to two flashes, not counting such as did not cross the field of view. These two flashes show complicated and beautiful structure. One of them is a multiple flash, distinctly seen as double by both my wife and myself. An enlargement of this shows curious flame-like appendages pointing upwards from every angle. The other flash is a broad ribbon. The images of the masonry in the left-hand corner (which are necessarily slightly out of focus) show three positions of the camera. They are sharp, hence the camera did not move during the existence of a flash; and the directions of those movements which did occur do not in any way correspond to the movements (if such there were) which would have been required to produce the ribbon-like effect from a linear flash.

A second plate shows four flashes, and the camera moved much more than in the first case. None of these flashes are ribbons. Development showed the plate to be overexposed.

The third was exposed to six flashes; that is to say, I judged that six of them crossed the field of view. There were many others between times, which were either in the clouds or occurred in other parts of the sky. One flash, I remarked at the time, must be "right down the middle of the plate." Development showed this plate to be very much overexposed, and the image required careful nursing. I was much surprised to see nothing but one triple flash in the corner. I supposed that I must have mistaken the plate, and was about to throw it away, but on carefully searching for the above-mentioned vertical flash, I found its image was reversed, printing as a black flash with a white core. Subsequent observation showed other dark flashes; and the enlargement of part of the plate shows that there are indications of white cores to each of them.

Now the connexion between this reversal and overexposure was very striking. Hence it occurred to me that the black flashes might be due to a sort of cumulative action, not to the excessive brightness of the individual flashes, but rather to the excessive action produced by the superposition of the glare from an illuminated white cloud upon the normal image of the flash.

To test this I endeavoured to obtain the same effect with

the sparks from a small Wimshurst machine; but, under the conditions in which I worked, I could not get a longer spark than one inch.

I first photographed a series of brilliant sparks, using two large Leyden jars. These gave normal images, very dense, and shaded off at the margins, although the focus, as shown by the knobs of the machine, was good.

Next I tried less brilliant sparks from the machine with its ordinary small jars. These gave similar images, but less dense.

Then I repeated both experiments, and before developing the plates exposed them to the diffused light from a gas-flame. The brilliant sparks then yielded images which may either be called normal with a reversed margin, or reversed with a normal core. The fainter sparks were completely reversed.

One plate of bright sparks was exposed to the gas-light, so that different parts were acted upon for different times. The reversal seems to spread inwards as the exposure to diffused light is increased.

One plate of faint sparks was only half of it exposed to diffused light. The result is that on that part the sparks are reversed, while on the other they are normal.

Finally I photographed a number of sparks in a series across the plate, and placed a sheet of white cardboard behind them to do duty for the white background of cloud. Some of the first sparks impressed on the plate show reversed images.

Coupling these experiments with the observations as to the overexposure of the "dark-flash" plate, and with the fact that all dark-flash plates I have seen show symptoms of considerable exposure, I submit that there is at least a good case for this theory of cumulative or repeated action producing the reversal. The partial reversal of the bright sparks seems to correspond with the bright core to some dark flashes; and the complete reversal of the less brilliant sparks to the absence of any such core from the less conspicuous portions of a dark flash.

There is certainly one difficulty yet to be got over, and that is the crossing of a dark flash by a bright one. However, I have some experiments* in view which I hope may throw some

* Since writing the above communication I have made a number of further experiments, which I hope to describe in detail at some future

light upon this also. In my own negative the point of crossing seems to be extra bright.

Meanwhile I must apologize to the Society for bringing forward these notes in such an immature and hastily constructed condition. My excuse must be that the photographs of electric sparks were only taken the day before yesterday, and today's meeting is the last of the session.

In the discussion which followed, Mr. W. N. Shaw exhibited a photograph taken during the same storm, which is particularly rich in dark flashes branching outwards from an intensely bright one. In some places the bright line has dark edges, and in one part a thin bright line runs along the middle of an otherwise dark portion of the flash. In answer to Mr. Inwards, Mr. Shaw said the plate was exposed about half a minute, and the former thought that under those conditions the appearance of the plate did not contradict Mr. Clayden's hypothesis.

Speaking of the same photograph, Prof. Perry considered that Mr. Clayden's observations would explain the result, for a bright flash required more exposure to diffused light to reverse it than a faint one did.

Prof. Ramsay reminded the meeting that Prof. Stokes's "oxides of nitrogen" explanation was still a possible one; and Mr. C. V. Burton asked whether they may be due to faint sparks cutting off light from brightly illuminated clouds, just as a gas-flame absorbs light from a brighter source.

In reply, Mr. Clayden thought the "oxides of nitrogen" hypothesis improbable, and said his experiments did not enable him to answer Mr. Burton's question. As regards Mr. Shaw's plate, he believed the diffused light from the clouds would be

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time. But perhaps I may be allowed to say at once that I have succeeded in imitating the phenomenon of a bright image crossing a dark one. The experiments point to the conclusion that diffused light acting upon a plate can reverse previously impressed images of electric sparks, but is powerless to affect any such impressions which may be made afterwards. Similar results are obtained whether the source of the diffused light is a gas-flame, a lamp, or a series of sparks. I do not at present offer any theoretical explanation of these facts, but they are in themselves sufficient from a meteorological point of view. "Dark" flashes of lightning have no existence in nature, but are caused by the exposure of the plate to an illuminated sky *after* the passage of the flash. This illumination may be due to subsequent flashes, the more recent of which will give normal images possibly crossing the reversed ones.

sufficient to reverse the fainter tributary flashes, although it was insufficient to reverse the primary one.

From data obtained when the ribbon-flash was taken he had made some calculations, which gave the height of the clouds about 1000 yards and the ribbon-flash 1300 yards long and 100 yards wide.

XXIX. *Expansion with Rise of Temperature of Wires under Pulling Stress.* *By* J. T. Bottomley, *M.A., F.R.S., F.C.S.**

[Plate V.]

It is probably well known to the members of the Physical Society that, at the instance of the British Association and with the assistance of a money grant from that body, very interesting secular experiments on the elasticity and ductility of wires were commenced some years ago in Glasgow. In the tower of the Glasgow University buildings certain wires are hung in pairs for comparison. One of each pair carries a heavy load about half the breaking weight of the wire; the other carries about one tenth of the breaking weight. Certain marks are put on the wires; and the object of the experiment is to find whether the heavily loaded wire seems, on comparison with the lightly loaded wire, to go on running down incessantly, or whether it comes asymptotically to a fixed length for a given temperature, ceasing to experience further permanent elongation.

The observations of the last few years show that the elongation due to further pulling out has, to say the least, become exceedingly small, so small that it is extremely difficult to observe it; and at the Aberdeen meeting of the British Association I pointed out that a great difficulty is introduced into the making of deductions from these observations through the impossibility of controlling the temperature of the tube in which the wires are placed. If, for example, there is any difference as to expansion with temperature of the same wire when lightly and when heavily loaded, a cause of disturbance would be introduced which it would be excessively difficult to allow for. It seemed therefore absolutely essential to make direct experiments on this point. The

* Read June 22, 1889.

object of the present communication is to give an account of some experiments of this kind. A preliminary account of these experiments was communicated to the British Association at the Manchester meeting (1887), and was printed in the Philosophical Magazine for October of that year.

The wires hung up in the tower of the Glasgow University building are two of platinum, two of gold, and two of palladium, these wires being chosen on account of their small liability to oxidation. The wires on which I have experimented up to the present time have, however, been of copper and platinoid. The latter metal is an alloy* of nearly the same composition as German silver, but containing a small quantity of tungsten and made in a peculiar way.

The figures show the arrangements for experimenting. A long tube of tin-plate about $2\frac{1}{2}$ inches in diameter was set up vertically, fixed by means of brackets at two or three places. This tube has inlets and outlets for steam, of which I have a plentiful supply in the laboratory from boilers connected with the University apparatus for heating and ventilation. It has also openings for thermometers. The length of the tube was $17\frac{1}{2}$ feet in the experiment with copper wire, and somewhat shorter in the platinoid experiment.

A piece of excellent copper wire was taken, and its breaking weight was found to be 750 grammes. Its diameter was 0·22 millim. Two portions of this wire were hung side by side in the centre of the tube. In order to suspend them their ends were passed into two small trumpeted holes in a stout brass plate and soldered to the back of the plate. The plate was screwed up to a strong beam in the ceiling of the laboratory. This forms by far the best mode of supporting a wire for experiments on elasticity. One of the wires carried 75 grammes, the other 375 grammes.

A few preliminary experiments as to heating and cooling revealed a difficulty the magnitude of which I was unprepared for. When the steam was admitted into the tube the wires of course expanded, the heavily loaded wire going down far more than the other; and when the steam had been stopped and the tube allowed to cool, they contracted again but not to the same extent; and neither came back to its

* Invented and patented by Mr. F. W. Martino of Sheffield.

original length. This was of course to be expected. But it turned out, on repeating the heating and cooling, that the same thing occurred again and again; and it was not till after about 150 heatings and coolings that the heavily loaded wire assumed a permanent state, expanding and contracting by equal amounts with the heating and cooling *. The lightly loaded wire took its permanent condition much sooner. This itself was a valuable result, applying directly to the early observations on the secular wires in the University tower.

Fig. 1, Pl. V., shows the arrangement for these preliminary experiments. Behind the wire a half-millimetre scale was put up; and each wire carried a pointer moving over the scale. The readings at hot and cold temperatures were taken with the well-known Quincke microscope-kathetometer; and the process and observations were carried on, as has been said, till each pointer gave unvarying readings at the hot and cold temperatures. It was then considered that the wires had assumed a permanent condition.

The pointers and scale were now removed and two hooks, of peculiar construction, figs. 2 and 3, were attached to the ends of the wire, the wires being passed into holes made for the purpose and soldered in. These hooks carried and formed part of the stretching weights. The upper parts of the hooks are turned over to form two horizontal plates, and the vertical parts of the hooks press very lightly against each other and form almost frictionless guides one for the other. In one of the vertical faces a vertical V-groove is cut, while the remainder of the face is plane and well-polished. Two little feet on the vertical face of the other hook move in the V-groove of the first, and a third foot rests against the smooth vertical face. A relative geometrical guide is thus provided for the hooks, and the shape of the hooks is such that the gravity of the whole, including the weights, gives the requisite slight pressure of the one against the other. The horizontal parts of the hooks just mentioned carry what is practically

* I must not fail to express here my indebtedness to Mr. Thomas A. B. Carver, assistant, and Mr. W. S. Cook, student in the Physical Laboratory, who carried out these experiments in the winter sessions 1887–8 and 1888–9 respectively. Without their patient labour the work would have been impossible to me.

a small three-legged table, of which two legs rest on one platform and the third on the other. To be more precise, one of the platforms carries on its top a little plate with a V-groove cut in it; and a knife-edge, cut away at the central parts and thus leaving two feet at the extremities, attached to the table, rests in the V. A third foot, rounded, rests on the other platform, which is plane and polished. On the top of this little table, which is a square of about $1\frac{1}{2}$ centimetre in the side, there is fastened a perfectly plane parallel Steinheil mirror; and a telescope with cross wires, looking down very nearly vertically on the mirror, views, reflected in the mirror, a half-millimetre scale suitably placed.

It will be seen at once that if the two wires were to elongate equally with rise of temperature, their extremities would go down together and almost the only effect (not absolutely of course) on the scale-reading would be to alter somewhat the focus. But if one wire elongates more than the other the mirror is tilted, and the change in the scale-reading readily gives the amount of relative displacement of the ends of the wire.

The arrangement works in the most satisfactory way, and it now only remains for me to state the results. I must remark, however, that it was exceedingly difficult to make an exact estimate of the temperature of the tube, even when the steam was running strongly through it. Thermometers inserted by means of corks in holes provided for the purpose showed that differences of 2° or 3° (I think not so much as 5°, however) existed at different parts of the tube.

This being understood, I may say that the range of temperature in the various experiments was from 15° C. or 16° C. (cold) to 98° or 99° (hot), or about 83° C. The length of copper wire experimented on was 530 centimetres. The difference of expansions observed was 0·14 millim. or 0·014 centimetre, the heavily loaded wire going down most. This gives a relative expansion of 26×10^{-6} per centimetre for a change of temperature of 83°; or $0{\cdot}314 \times 10^{-6}$ per centimetre per degree.

I find the linear expansion of copper per degree stated at about $17{\cdot}2 \times 10^{-6}$, and thus the ratio of this extra expansion to the total expansion is 3·14/172, or about $\frac{1}{55}$.

With regard to platinoid wire—after more than three months of daily heating and cooling, the wires (0·35 millim. in diameter) came to a thoroughly permanent condition. On a length of 490 centimetres a relative extra extension of 0·111 millim. or 0·011 centim. was observed for a change of temperature of 83°, and as with copper the heavily loaded wire experienced most elongation. These numbers give $22{\cdot}4 \times 10^{-6}$ as the extra expansion per centimetre, or $0{\cdot}27 \times 10^{-6}$ per centimetre per degree Centigrade.

The linear expansion of platinoid was unknown, though it might be supposed to be something not very different from that of German silver. Accordingly a series of experiments were carried out on this question, with the result that the linear expansion of the specimen used was found to be ·0000154 per degree Centigrade.

The relative extra expansion of platinoid wire is therefore $\frac{2{\cdot}7}{154}$, or $\frac{1}{57}$.

Mr. H. Tomlinson thought the probationary period for copper might be considerably shortened by repeatedly putting on and taking off the load, and by subjecting the wire to torsional oscillation. With iron wires this would not be the case, for they behave in a most peculiar manner, and require long periods of rest after each oscillation. From experiments he had conducted during the last two years, he found that the permeability of iron could be enormously reduced by repeated heatings and coolings whilst undergoing magnetic cycles of small range.

Mr. Gregory said the paper threw considerable light on some experiments on the lag of stretched wires upon which he was engaged. He also suggested heating the wires by electric currents.

In reply, Mr. Bottomley said he had considered it important to leave the wires untouched after being suspended; and, as regards heating by electricity, he thought that convection-currents would make the temperature non-uniform.

XXX. *On a Relation existing between the Density and Refraction of Gaseous Elements and also of some of their Compounds.* *By* Rev. T. Pelham Dale, *M.A.**

In my former paper which I presented to the Society† I touched upon the empirical relations discoverable between the specific refractive energies of selenium and sulphur. The present state of the data, however, relating to these substances is so far unsatisfactory that we are not furnished with refractive indices and density taken from the same specimen at the same temperature. Thus there is a doubt as to the density to be chosen as normal, in consequence of the difference due to the allotropic conditions in which these substances are found. This difficulty may be to a certain extent eluded by taking both densities and calculating the specific refractive energy for both. The result is, in the case of selenium and sulphur, that in both substances the mean values are not very far apart.

The question, however, of the relation between the refractive indices of the elements is of so great interest, that it appeared advisable to calculate the value of $\frac{\mu-1}{d}$ of all chemical elements for which data existed, giving both refractive index and density in the state of gas or vapour. This would include the refractive indices of hydrogen, oxygen, and nitrogen, and the importance of these is obvious. Then, again, the calculated densities derived from atomic weights might be used in cases where an observation was wanting, and as a check where these existed. A very few trials revealed relations which it was impossible to overlook.

These relations among the numbers found are set forth in the accompanying Tables.

These tables are arranged in columns. The first, column I., contains the name of the substance. Column II. its index of refraction less unity, or $\mu-1$. Column III. its density. Column IV. its specific refractive energy, or $\frac{\mu-1}{d}$. Column V. the ratio of $\mu-1$ in the substance to the similar number for hydrogen. The upper row of numbers in each line are natural numbers, and those immediately under are the man-

* Read May 25, 1889. † *Ante*, p. 17.

tissæ of their common logarithms. Examining this table in detail, it is seen at once that the logarithms of the specific refractive energies of nitrogen, chlorine, and phosphorus are nearly identical, as also of sulphur and oxygen. It will also be observed that in N, Cl, P this log. is double of that of H, and S and O three times that of half the log. specific refractive energy of hydrogen. In the same way, the log. of the specific refractive energy of mercury is 8 times, and arsenic 9 times this quantity.

If we turn to column V. we find that oxygen has nearly double the refractive energy of hydrogen, mercury 4, arsenic 8, sulphur, but not so closely, 12 times.

Now all these coincidences arise from observation only, and are independent of theory altogether. The probability that they should be fortuitous is very small in so large a number of instances.

When we turn to the compounds, we again see indications of the same law. Thus N_2O has log. specific refractive energy half of similar log. of CO ; and the refractive energy of Cy is very nearly 6 times that of hydrogen. All these numbers may, roughly it is true, be united under a single law: thus, $\frac{\mu-1}{d}$ for hydrogen $=15696$, its logarithm is 19578, and half this is ·09789 ; this is according to Prof. Everett's data. Mr. Lupton's give ·09374. If we multiply these successively by 1, 2, ... and 10, we shall find that almost all the logarithms range between these two products, being less than the greater and greater than the smaller. It is worth noticing that $\frac{\pi}{2} = 1{\cdot}57079$, $\log \frac{\pi}{2} = 19612$—a curious coincidence which, if quite fortuitous, will nevertheless prove a help to the calculator.

I have not as yet attempted to express these relations under an algebraic formula, although it is obvious that it might be very readily done. I prefer to call attention to the existence of these empirical relations, which hitherto seem to have escaped notice.

In some of the instances set forth in the Tables the density was checked by calculation from the equivalents. This also, it appears, opens another field of great interest. The well-known relations between molecular weight and density lead us to expect a relation between molecular weight and refrac-

tion, and this relation has been abundantly worked out in the case of liquids by my friend Dr. Gladstone. I have not as yet had time to work out this part of the subject completely, as though the calculations necessary present no difficulty they require a numerous and bulky array of figures which must be carefully verified and checked, and also because more data are needed. It is also unfortunate that no data exist, which are accessible to me, of the absorption-spectra of chlorine, bromine, or iodine, or of sulphur and selenium, comparable with the observations here used as the basis of the calculations in the Table. It is in the hope that some of these data may be supplied that I venture to put this paper before the Society.

Some interesting relations are observable between the equivalents and refractions* of gaseous elements and compounds, which I hope to present in a future paper.

Note.—The data are taken from Prof. Everett's 'Units and Physical Constants,' 2nd ed. 1886 (marked E); and from Mr. Lupton's Numerical Tables, 1884 (marked L).

I. Substance.	II. Refraction, $\mu-1$.	III. Density. d.	IV. Sp. Ref. $\frac{\mu-1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
H.	0001387	00008837	15696	1·00	
	14208	94630	E 19578		L 18749 Another value of log $\frac{\mu-1}{d}$ Lupton's data of log.
O.	2706	14107	19182	1·95	
	43233	14943	E 28290	29025	L 27923 do.
N.	2977	12393	24241	2·14	$\frac{\mu-1}{d}$
	47378	09318	E 38060	33170	378134 do.
Cl.	L 7720	E 30909	24986	4·27	} Density given by Everett.
	88762	49009	39753	62992	
Cl.	L 7720	L 3·18	24274		} Density and Index according to Lupton.
		50243	38159		
S.	L 16290	96×H	19202	11·7	} Density according to Gmelin.
	1·21192	98227	28335	06984	
P.	L 13640	62×H	24895	9·84	} Density calculated from equivalent.
	1·13481	73869	39612	99273	
Hg.	L 5560	100×H	62918	4·00	} Calculated from hydrogen.
	74507	94630	79877	60299	
As.	L 11140	150×H	74268	8·03	} Ditto.
	1·04689	17609	87080	90479	

* I propose to call the quantity $\mu-1$ the refraction, μ the index.

Compounds.

I. Substance.	II. Refraction. $\mu-1$.	III. Density. d.	IV. Sp. Ref. $\frac{\mu-1}{d}$.	V. Ratio of Refraction to that of Hydrogen.	Remarks.
N_2O.	E 2975	E 19433	15309	2·14	All these are calculated from experimental data of both index and density from Prof. Everett.
	1·47349	28554	18495	33141	
NO.	E 5159	E 13254	38924	3·72	
	71257	12235	59022	33141	
CO.	E 3350	E 12179	22947	3·27	
	52504	29024	36720	57049	
SO_2	E 7036	E 26990	26070	5·07	
	84733	43120	41613	70525	
Cy.	E 8216	E 22990	35737	5·92	
	91466	36154	55312	77258	
NH_3.	385	0·761		2·78	6505
	5855	8814	7041	4434	
HCl.	449	1·64		3·24	3969 Calculated from Lupton's numbers.
	6522	2148	4484	5107	
H_2S.	665	1·52		4·79	
	8228	1818	6410	6807	
CH_4.	443	Calc.		2·02	Density calculated from hydrogen and equivalent.
	6464	8554	7916	3040	
C_2H_4.	678	Calc.		4·9	
	8312	8554	7328	6900	

These to four places are evidently of less value than those above.

Note by Prof. A. W. Rücker, *F.R.S.*, *on* Mr. Dale's *Paper*.

It has been shown that the volume of the molecules in unit volume of the substance which they form is $(\mu^2-1)/(\mu^2+2)$, where μ is the refractive index.

If μ is nearly equal to unity (as in the case of the gases) this expression reduces to $\frac{2}{3}(\mu-1)$. Hence if δ is the density of the body, v and m the volume and mass of a molecule and n the number of molecules in unit volume,

$$\frac{\mu-1}{\delta} = \frac{3}{2}\frac{nv}{nm} = \frac{3}{2}\frac{v}{m}.$$

For the same substance the right-hand side should be a constant, and it has been shown that it nearly fulfils this condition.

Mr. Dale now states that for different substances

$$\log\frac{\mu-1}{\delta}=ac,$$

where c is a constant independent of the nature of the substance, and a is an integer. This at once leads to the relation $v/m=\frac{2}{3}e_{ac}$, which would indicate that the ratios of the volumes to the masses of the molecules are in geometrical progression, or, more shortly, the densities of the molecules are in geometrical progression.

If, then, Mr. Dale's conclusions are correct this would be the theoretical inference to be drawn from them.

XXXI. *Notes on Geometrical Optics.*—Part I.

By Professor SILVANUS P. THOMPSON, *D.Sc.**

1. *The Deduction of the Elementary Theory of Lenses and Mirrors from Wave Principles.*

INTRODUCTORY.—The division of Optics into a "geometrical" part, founded upon the treatment of the subject from the ray point of view, and a "physical" part, founded upon the treatment of the subject from the wave point of view, has long seemed illogical. Experience in the teaching of the science suggests that it would be preferable to adopt the wave theory as a common basis, provided the formulæ of lenses and mirrors which form the staple of geometrical optics are as readily established on wave principles as on ray principles. Five years ago the author of these notes made an attempt to rewrite the elementary part of geometrical optics on wave principles; and though hitherto he has published nothing on the subject, he has subjected the method to the test of experience, and has made it the basis of his optical lectures year by year. The form finally adopted, and now for the first time set forth, has been modified from time to time and simplified. As a result, whilst the formulæ

* Read June 8, 1889.

for second approximations, aberration and the like, can be deduced with no greater difficulty than in the orthodox way, the elementary formulæ for first approximations are much more simply deduced, and do not even require the use of trigonometrical functions. This is the more striking because, as all teachers of the subject know, the very first assumptions made in establishing the elementary formulæ of spherical mirrors and lenses are that angles may be written instead of their sines or tangents. The assumptions made in the method now published are, it is true, different, but involve no greater sacrifice of accuracy, the formulæ being true to the same degree of approximation, and, indeed, though written in a different notation, are in substance identical with those in common acceptance.

It will be convenient, first, to set forth the general bases of the method; next to define the method of reckoning curvatures; then to explain the notation employed; lastly, to develop the elementary formulæ.

2. *General Bases.*

In treating optics from the new standpoint, we have to think about surfaces instead of thinking about mere lines. Waves march always at right angles to their surfaces; a change in the form of the surface alters the direction of march. The wave-surface is to be considered instead of the "ray." The curvature of the surface therefore becomes the all-important consideration. All that any lens or mirror or any system of lenses or mirrors can do to a wave of light is to imprint a curvature upon the surface of the wave. If the wave is initially a plane wave, then the curvature imprinted upon it by the lens or mirror will result in making it either march toward a point (a real focus) or march as from a point (a virtual focus). If the wave possesses an initial curvature, then all that the lens or mirror can do is to imprint another curvature upon its surface, the resultant curvature being simply the algebraic sum of the initial and the impressed curvatures. As will be seen, in the new method the essential thing to know about a lens or mirror is the curvature which it can imprint on a plane wave: this is, indeed, nothing else than what the opticians call its "power;" the focal power

being inversely proportional to the so-called focal length. Another, but less vital point in the method, is the abandonment of the use of the so-called index of refraction in favour of a quantity reciprocally related to it, and here denominated the velocity-constant. The use of the index of refraction dates from a time anterior to the discovery that refraction was a mere consequence of the difference of velocity of the waves in different media. The index of refraction is a mere ratio between the sines (or originally the cosecants) of the observed angles of incidence and refraction. The uselessness of clinging to it as a foundation for lens formulæ is shown by the simple fact that, in order to accomplish the very first stage of reasoning in the orthodox way of establishing the formulæ, we abandon the sines and write simply the corresponding angles, as Kepler did before the law of Snell was discovered. The elementary formulæ of lenses are, in fact, where Kepler left them. It is now common knowledge that the speed of light, on which refraction depends, is less in optically dense media than in air. The speed of light in air is not materially different from one thousand million feet per second, or thirty thousand million centimetres per second. If we take the speed of light in air as unity, then the numeric expressing the speed in denser media, such as glass or water, will be a quantity less than unity, and will differ for light of different wave-lengths. It is here preferred to take the speed of light in air, rather than *in vacuo*, as unity, because lenses and optical instruments in general are used in the air. The numeric expressing the relative velocity in any medium is called its "velocity-constant"; it is the reciprocal of the index of refraction. The velocity-constant, for mean (yellow) light, for water. is about 0·75; that of crown glass 0·65; that of flint glass from 0·61 to 0·56, according to its density.

3. *Method of Reckoning Curvature.*

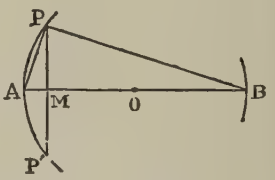
The Newtonian definition of curvature as the reciprocal of the radius has a special significance in the present method of treating optics: for some of the most important of lens and mirror formulæ consist simply of terms which are reciprocals of lengths, that is to say of terms which are

curvatures. The more modern definition of curvature as rate of change of angle per unit length of the curve (Thomson and Tait's 'Natural Philosophy,' ii. p. 5) is equivalent to Newton's; for if in going along an arc of length δs, the direction changes by an amount $\delta\theta$, the curvature is $\delta\theta/\delta s$. But the angle $\delta\theta = \delta s/r$, where r is the radius of curvature; hence the curvature $= \delta s/r\delta s = 1/r$.

There is, however, another way of measuring curvature, which, though correct only as a first approximation, is eminently useful in considering optical problems. This way consists in measuring the bulge of the arc subtended by a chord of given length.

Consider a circular arc AP, having O as its centre. Across this arc draw a chord PP′ of any desired length. The diameter A B bisects it at right angles in M. The short line M A measures the depth of the curve from arc to chord. If the radius is taken as unity the line M A is the versed-sine of the angle subtended at B by the whole chord, or is the versed-sine of the semi-angle subtended at the centre. In Continental works it is frequent to use the name *sagitta* for the length of this line M A; and as this term is preferable to versed-sine, and can be used generally irrespective of the size of radius, it is here adopted. The proposition is that, for a given chord, the *sagitta* is (to a first degree of approximation) proportional to the curvature. For it follows from the construction that

Fig. 1.



$$\mathrm{MA} \cdot \mathrm{MB} = (\mathrm{PM})^2;$$

assuming PM as unity,

$$\mathrm{MA} = \frac{1}{\mathrm{MB}} = \frac{1}{2r - \mathrm{AM}}.$$

But, for small apertures, AM is small compared with $2r$, and may be neglected in the denominator, whence

$$\mathrm{MA} \doteqdot \frac{1}{2} \cdot \frac{1}{r}.$$

Twice* the sagitta represents numerically the curvature. The error is less than 1 per cent. when the semi-angle subtended at the centre is 10°; less than two per cent. when it is 15°; less than five per cent. when it is 25°.

If the method of reckoning curvatures by means of the sagitta required justification, that is afforded by the fact that the practical method of measuring the curvatures of lenses and mirrors by the *spherometer* consists essentially in applying a micrometer-screw to measure the sagitta of the arc subtended by a fixed chord, the diameter of the contact circle drawn through the three feet of the instrument. In this case, as indeed in all cases where accuracy, not approximation, is desired, the basis for calculation of the correction exists in the actual size of the diameter of the contact circle, which is a fixed parameter for all measurements made with the instrument.

The sign of the curvature remains to be defined. In the case of actual waves of light, the sign adopted will be + for the curvature of waves which are converging upon a real focus; — for those which are diverging either from a luminous source or from a virtual focus. This agrees with the practice of the ophthalmists and of the opticians, who always describe a converging lens as positive. A positive lens is one which imprints a positive curvature upon a plane wave which traverses it.

The *unit of curvature*, whether of the wave-surface itself or of the surface of any mirror or lens, will be taken so as to accord with modern ophthalmic and optical practice as *the dioptrie*; that is to say, the curvature of a circle of one metre radius will be taken as unity. The dioptrie, originally proposed by Monoyer as the unit of focal power of a lens, was formally adopted in 1875 by the International Medical Congress at Brussels, and its great convenience has led to its universal adoption for the enumeration of the focal powers of lenses. That lens which has a focal length of 1 metre is said to have a focal power of one dioptrie. In other words, such

* Though the sagitta is numerically *half* the curvature, since all the formulæ of first approximation are homogeneous and of the first degree as regards sagittæ and curvatures, the numerical factor $\frac{1}{2}$ disappears in passing from sagittæ to curvatures, or *vice versâ*.

a lens prints a curvature of one dioptrie upon a plane wave which is incident upon it. For the present proposal to extend the use of the term from focal powers (*i. e.* imprinted wave-curvatures) to the curvatures of curved surfaces in general, the writer is responsible.

4. *Notation.*

In adopting a notation which embodies the new method it is obviously advisable to choose one which lends itself most readily to the existing and accepted notations. In the great majority of books on optics, the recognized symbol for focal length is f; that for radius of curvature r. And in the Cambridge text-books for many years the distances from lens or mirror of the point-object and the point-image have respectively been designated by the letters u and v. Now it is the reciprocals of these which occur in the expressions for the curvatures of surfaces or of waves. The symbols adopted respectively for the four reciprocals are accordingly $\mathscr{F}$, $\mathscr{R}$, $\mathscr{U}$, and $\mathscr{V}$. The accepted symbol for the index of refraction is the Greek letter μ; for the velocity-constant, which is its reciprocal, we take the letter h. The following is a tabular statement of the symbols and their meanings:—

Symbol.	Meaning.	Equivalent in Current Notation.
$\mathscr{F}$	Focal curvature, or Focal power of lens or mirror (= dioptries, if metre is taken as unit of length)	$\frac{1}{f}$
$\mathscr{R}$	Curvature of Surface	$\frac{1}{r}$
$\mathscr{U}$	Curvature of Incident wave; *i. e.* curvature which it has acquired by having travelled from point of origin ("incident focus") to incidence	$\frac{1}{u}$
$\mathscr{V}$	Curvature of Resultant wave; *i. e.* curvature with which wave emerges from the lens	$\frac{1}{v}$
h	Velocity-Constant of medium; *i. e.* velocity of light in that medium compared with velocity in air taken as unity	$\frac{1}{\mu}$

Expansion of Curvatures.—If the curvature $\mathscr{R}$ of a wave at any point is known it is easy to calculate the curvature at any other point at distance d further from or nearer to the centre, the formula for the new curvature $\mathscr{R}'$ being as follows:—

$$\mathscr{R}' = \mathscr{R} \frac{1}{1 \pm \mathscr{R} d} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The + sign must be taken where the new point is further from the centre than the point for which the curvature $\mathscr{R}$ is specified; the − sign when it is nearer the centre. This proposition is of use in dealing with thick lenses, and with thin lenses at a given distance apart.

5. *Refraction Formulæ.*

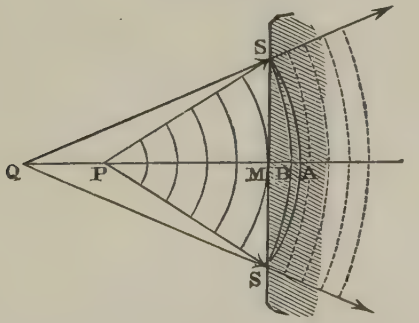
As a preliminary to lens formulæ, it is convenient to consider certain cases of refraction.

A. Plane Surface: Curved Wave.

Case (i.) *a. Entrant Wave of Negative Curvature.*

Consider a retarding medium, such as glass, bounded on the left (fig. 2) by a plane surface S S. Let P be a source of

Fig. 2.



waves incident on the surface, PM being a line perpendicular to S S. The wave-fronts, at successive small intervals of time, are represented by arcs of circles. At a certain moment the wave, had it been going on in air, would have had for its surface the position SAS; the curvature being measured by the sagitta AM. The medium, however, retards the wave,

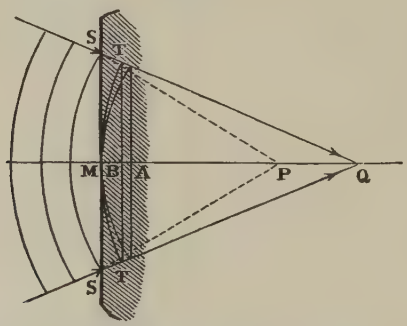
and it will only have gone as far as B instead of penetrating to A; B being a point such that $BM = h \cdot AM$, where h is the velocity-constant of the medium into which the wave enters. The curvature of the wave is flattened as the result of the retardation. Now draw a circle through SBS, and find its centre Q. To a first degree of approximation the arc SBS represents the retarded wave-front, the set of wave-fronts from B onwards being represented by the series of arcs drawn from Q as centre. An eye situated in the medium on the right of SS will perceive the waves as though coming from Q, the (virtual) point-image of P. Accurately the wave-fronts should be hyperbolic arcs, but if S S is small relatively to P M the circular arcs are adequate. Now $AM = \mathscr{U}$, and $BM = \mathscr{V}$. Hence the action of the plane surface upon the curvature of the incident wave is given by the formula

$$\mathscr{V} = h\mathscr{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Case (i.) *b*. *Entrant Wave of Positive Curvature.*

The entrant wave (fig. 3) has a positive curvature or convergence such as would cause it to march to the point P (the

Fig. 3.



incident focus) if its path lay wholly in air. At a certain moment, when the middle point of the wave-front has reached M, the outer portions of the wave-front passing in through S S would have reached positions as far as the vertical line drawn through A had the path lain wholly in air. But being retarded they only reach as far as the line T B T drawn through B; where again $BM = h \cdot AM$. The circular arc through T B T has Q for its centre; that is to say, after

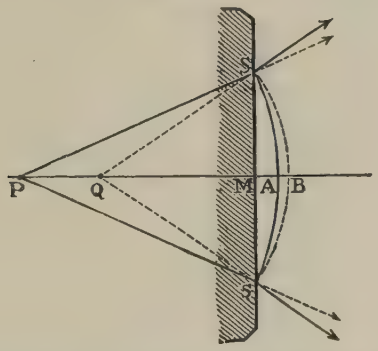
entry the waves now converge on Q instead of P. In this case also the effect on the wave of entrance into the retarding medium is to flatten its curvature, and the formula is as before,

$$\mathcal{V}=h\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)\,bis.$$

Case (ii.) *a*. *Emergent Wave of Negative Curvature.*

Consider the wave emerging (fig. 4) into air from a point P, situated in the retarding medium whose velocity-constant is h. Had the wave been going on wholly through the denser

Fig. 4.



medium, the wave-front would have been at S A S; but, being accelerated on emergence into air, it reaches B instead of A. The new curve S B S has Q for its centre; that is to say, the wave emerges from Q as a virtual focus, its curvature being augmented. The sagitta B M is greater than A M in the ratio of 1 to h. Hence in this case the formula is

$$\mathcal{V}=\frac{1}{h}\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Case (ii.) *b*. *Emergent Wave of Positive Curvature.*

There is no need to prove this case separately; it leads to the same formula

$$\mathcal{V}=\frac{1}{h}\mathcal{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)\,bis.$$

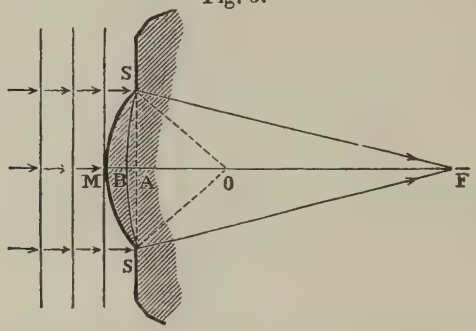
In the case of either positive or negative initial curvature, emergence from the retarding medium through the plane surface into air augments the curvature.

B. Curved Surface: Plane Wave.

Case (i.) *a*. *Entrant Wave; Convex Surface.*

The surface S M S is convex toward the light; the centre of curvature being O. Consider the plane wave entering the surface. At a certain instant it would have arrived at S A S had its path lain wholly in air. Because of the retarding medium the central portion of the wave-front will only reach B instead of A; B being such a point that $B M = h . A M$. As a result the wave is imprinted with the curved surface S B S, the centre of which is at F, toward which point the

Fig. 5.



wave converges. The sagitta A B measures the focal curvature thus impressed on the wave. Now

$$A B = A M - B M$$

$$= A M - h . A M,$$

or

$$\mathscr{F} = \mathscr{R} (1 - h). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It will be noted that if the curvature of the surface is positive (*i. e.* bulging toward the source of light), the impressed focal curvature is also positive. For the case of any two media having respective velocity-constants h_1 and h_2, the formula becomes

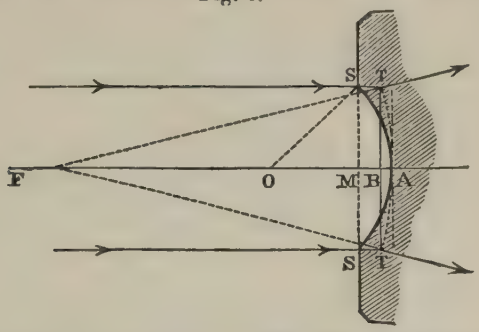
$$\mathscr{F} = \mathscr{R} \frac{h_1 - h_2}{h_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Case (i.) *b*. *Entrant Wave; Concave Surface.*

Consider the plane wave which would have reached the vertical line though A had its path lain wholly in air; the

central portion does reach A, but the marginal portions are retarded, and only reach the vertical line T B T drawn through

Fig. 6.



B, a point such that $BM = h \cdot AM$. The impressed curve is T A T with centre F. That is to say, the concave (or negative) surface imprints a negative focal curvature on the wave, its sagitta being A B.

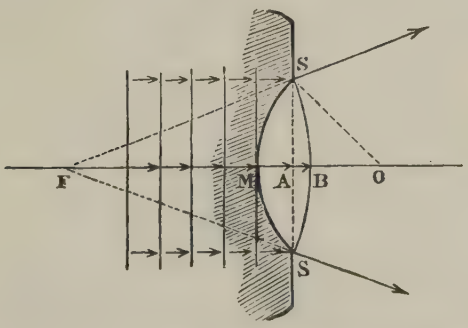
$$AB = AM - BM$$
$$= AM - h \cdot AM,$$
$$\mathscr{J} = \mathscr{R}(1-h). \quad . \quad . \quad . \quad . \quad . \quad (4)\ bis$$

The formula, therefore, is the same for entrant plane waves whether the surface be convex or concave, the sign of $\mathscr{J}$ following the sign of $\mathscr{R}$.

Case (ii.) *a*. *Emergent Wave; Surface Convex toward light* (*i. e.* concave toward air into which wave emerges).

The plane wave would have had its front at S A S (fig. 7)

Fig. 7.



at a certain instant had its path lain wholly in the retarding

medium; but the central portion being accelerated by its emergence at M into air reaches B, where B M is to A M as 1 to h. Hence the curve S B S, whose curvature is measured by the sagitta A B, is the impressed focal curvature. It will be noted that this is of opposite sign to that of the surface of emergence. Hence the sagitta A B must be taken with minus sign.

$$\begin{aligned} -\mathrm{AB} &= \mathrm{BM} - \mathrm{AM} \\ &= \frac{1}{h}\mathrm{AM} - \mathrm{AM} \\ +\mathrm{AB} &= \mathrm{AM}\left(1 - \frac{1}{h}\right), \\ \mathscr{F} &= \mathscr{R}\left(\frac{h-1}{h}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6) \end{aligned}$$

As before, for any two media having respective velocity-constants h_1 and h_2, the formula becomes

$$\mathscr{F} = \mathscr{R}\frac{h_1 - h_2}{h_1}, \quad . \quad . \quad . \quad . \quad . \quad (5)\,bis.$$

which, in the present case where $h_1 < h_2$, will give $\mathscr{F}$ of opposite sign to $\mathscr{R}$.

Case (ii.)*b*. *Emergent Wave; Surface Concave toward light* (*i. e.* convex toward air into which wave emerges).

This is similar to the preceding, and yields the same formula.

Comparison of Case (i.)*a with Case* (ii.)*b*.

Comparing formula (4) with formula (6), we get for the two primary focal curvatures impressed respectively on plane waves passing in opposite directions through the curved surface,

$$\frac{\mathscr{F}_1}{\mathscr{F}_2} = -\frac{h_2}{h_1}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

whence, for the two primary focal lengths,

$$\frac{f_1}{f_2} = -\frac{h_1}{h_2}.$$

The focal lengths are, as the negative sign indicates, to be measured in opposite directions with respect to the surface. Also, taking the algebraic sum of the two primary focal

lengths, we get

$$f_1 = r\frac{h_1}{h_1 - h_2},$$

$$f_2 = r\frac{h_2}{h_2 - h_1} = r\frac{-h_2}{h_1 - h_2},$$

$$f_1 + f_2 = r\frac{h_1 - h_2}{h_1 - h_2} = r:$$

and, as one of the focal lengths is negative, it follows that the difference of their lengths is equal to the radius of the curved surface.

C. Curved Surface: Curved Wave.

The cases in which a wave possessing initial curvature passes through a curved surface and acquires a resultant curvature may be dealt with, apart from any further geometrical constructions, by applying the principle of superposition of curvatures. Thus, take the case of a wave possessing initial curvature $\mathscr{U}$ entering from air into a medium having velocity-constant h, and so curved that the focal power of the curved surface is $\mathscr{F}$. Then, as the wave enters the surface of the medium two effects will occur: its initial curvature will be altered in the ratio of the velocities, and there will be superposed upon it the focal curvature of the surface; or, in symbols,

$$\mathscr{V}_1 = h\mathscr{U} + \mathscr{F}_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For an emergent wave, possessing initial curvature $\mathscr{U}$ in the medium, the formula will be

$$\mathscr{V}_2 = \frac{1}{h}\mathscr{U} + \mathscr{F}_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

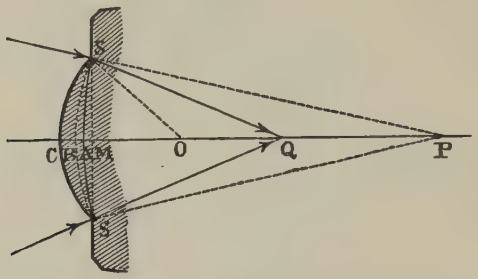
Or, for the case of a wave passing from a medium of velocity-constant h_1 to another of velocity-constant h_2, the formula will be

$$\mathscr{V} = \frac{h_2}{h_1}\mathscr{U} + \mathscr{F}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

It is easy, however, to prove any one of the several cases that may arise, without in this way relying upon the principle of superposition. Take the case of a positive wave entering a positively curved surface.

Let S C S (fig. 8) be the surface of the medium, its curvature being measured by the sagitta C M. There will be

Fig. 8.



a certain moment when the entrant wave, converging toward P, would have had as its front S A S had its path lain wholly in air. But the central portion has entered the retarding medium at C, hence will only have advanced as far as B instead of A; B being such that $BC = h \cdot AC$. Hence the resultant wave will have the form S B S, and the sagitta of the resultant curvature is B M.

$$\begin{aligned} BM &= CM - CB, \\ &= CM - h \cdot AC, \\ &= CM - h(CM - AM), \\ &= CM(1-h) - h \cdot AM; \end{aligned}$$

$$\mathscr{V} = \mathscr{R} - h\mathscr{R} + h\mathscr{U}.$$

But
$$\mathscr{R}(1-h) = \mathscr{F},$$

whence
$$\mathscr{V} = \mathscr{F} + h\mathscr{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)\,bis$$

D. Lens Formulæ.

Case i. *Thin Lens; Plane Wave.*

In the case of a lens, the curvature $\mathscr{F}_1$ imprinted on a plane wave by entrance at the first surface may be regarded as an initial curvature of the wave which emerges through the second surface. Emergence into air will, as shown above, alter the curvature by augmenting it in the ratio of 1 to h, and superpose upon it the focal curvature $\mathscr{F}_2$ due to the second surface. Hence the whole resultant curvature $\mathscr{F}$

imprinted by a *thin* lens on the plane wave will be

$$\mathscr{F} = \frac{1}{h}\mathscr{F}_1 + \mathscr{F}_2$$

But

$$\mathscr{F}_1 = \mathscr{R}_1(1-h),$$

and

$$\mathscr{F}_2 = -\mathscr{R}_2\left(\frac{1-h}{h}\right);$$

whence

$$\mathscr{F} = \mathscr{R}_1\frac{1-h}{h} - \mathscr{R}_2\frac{1-h}{h},$$

or

$$\mathscr{F} = (\mathscr{R}_1 - \mathscr{R}_2)\frac{1-h}{h}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This formula may be compared with that in the current notation,

$$\frac{1}{f} = \left\{\frac{1}{r_1} - \frac{1}{r_2}\right\}(\mu - 1).$$

In the case of a lens composed of a medium h_2, lying between two other media h_1 and h_3, the formula becomes

$$\mathscr{F} = \frac{1}{h_1 h_2}\{\mathscr{R}_1(h_1 - h_2)h_2 + \mathscr{R}_2(h_2 - h_3)h_1\}. \quad . \quad (12)$$

Case ii. *Thick Lens; Plane Wave.*

If the lens has thickness d, the rule for expansion of curvature at end of § 4 above at once gives

$$\mathscr{F} = \mathscr{F}_2 + \frac{1}{h}\mathscr{F}_1\frac{1}{1 \pm \mathscr{F}_1 d}, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

or

$$\mathscr{F} = \left\{\mathscr{R}_1\frac{1}{1 \pm \mathscr{R}_1(1-h)d} - \mathscr{R}_2\right\}\frac{1-h}{h}. \quad . \quad (14)$$

Case iii. *Any Lens; Any Wave.*

The principle of superposition at once gives the universal formula for all lenses bounded by identical media on the two sides :—

$$\mathscr{V} = \mathscr{U} + \mathscr{F}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or, in words, the resultant curvature is the algebraic sum of the initial curvature and the impressed curvature. This may

again be compared with the formula in current notation:

$$\frac{1}{v} = \frac{1}{f} - \frac{1}{u}.$$

The difference in sign attributed to the term $\frac{1}{u}$ arises from conventions adopted in the two systems.

E. Two Thin Lenses at distance apart.

The principle of expansion of curvature at once gives us as the equivalent focal power,

$$\mathscr{F} = \mathscr{F}_2 + \mathscr{F}_1 \frac{1}{1 + \mathscr{F}_1 d}, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where $\mathscr{F}_1$ and $\mathscr{F}_2$ are the focal powers of the first and second lenses, and d the distance between them. $\mathscr{F}$ will be in *dioptries* if $\mathscr{F}_1$ and $\mathscr{F}_2$ are in *dioptries* and d in metric units. If the two thin lenses are close together, the resultant power is simply the algebraic sum of the powers of the separate lenses. One simply adds the *dioptries* of the separate lenses to find the resultant *dioptries*.

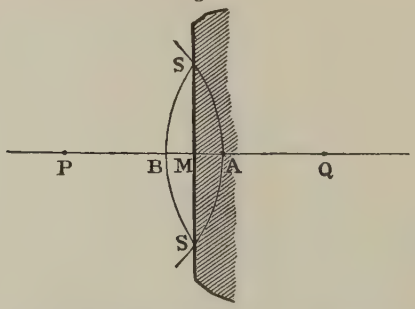
6. *Reflexion Formulæ.*

Mirrors.

Case i. *Plane Mirror; Curved Wave.*

The mirror (fig. 9) has surface S M S. The incident wave would have had front S A S at a certain instant had its path lain wholly in air. The central portion of the wave, which would have reached A, travels backwards to B, an equal distance, in

Fig. 9.



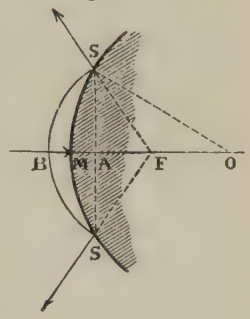
the same time. The sagitta B M of the resultant curvature is equal to and of opposite sign to the sagitta A M of the initial curvature; or

$$\mathscr{V} = -\mathscr{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Case ii. *Curved Mirror; Plane Wave.*

There are two cases, equally simple, of convex and concave mirrors. One will suffice. Consider (fig. 10) a plane wave which at a certain instant would have arrived at S A S had its path lain wholly in air. The central portion of the wave has, however, struck at M, and marches backwards to B in same time as it would have taken to reach A. Hence

Fig. 10.



$$BM = AM,$$

or

$$BA = 2AM.$$

But A M measures the curvature of the mirror, whilst B A measures the curvature impressed on the plane wave. Hence

$$\mathscr{J} = 2\mathscr{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

Case iii. *Any Mirror; Any Wave.*

The principle of superposition at once leads to a general formula, expressing the sum of the two actions of the mirror on the wave; it reverses its initial curvature, and then imprints a focal curvature upon it. In symbols,

$$\mathscr{V} = -\mathscr{U} + \mathscr{J}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

7. *Formula of the Lenticular Mirror.*

The lenticular mirror consists of a lens one face of which is silvered, so that the light passes twice through the lens, and suffers an intermediate reflexion at the silvered surface. If the focal power of the lens be $\mathscr{J}_1$, and the curvature of the mirror-surface be $\mathscr{R}$, then the total curvature which the system impresses upon a plane wave will be equal to $2\mathscr{J}_1 + 2\mathscr{R}$, and there will be a reversal of the direction of the light, so that

$$\mathscr{J} = -2(\mathscr{J}_1 + \mathscr{R}). \quad . \quad . \quad . \quad . \quad . \quad (19)$$

8. *A Dioptrie Spherometer.*

In order the better to bring to practical issues the method of treating lens problems by curvatures, the author designed

a special spherometer. In this instrument the readings of the scale are caused to indicate directly the dioptries of the curvature; that is to say, one whole turn of the micrometric screw corresponds accurately to a curvature having one metre as its radius. In the particular instrument now exhibited the pitch of the micrometric screw is 1 millimetre. To make this correspond to a curvature of one dioptrie, the radius of the contact-circle on which the three feet stand must be 44·71 millimetres. For, by reference to fig. 1, it will be seen that the radius PM of the contact-circle is such that

$$(\mathrm{PM})^2 = \mathrm{AM}\,.\,\mathrm{MB} = 2rs - s^2,$$

where s is the sagitta of the curve. Hence, if $r = 1000$ millimetres, and $s = 1$ millimetre, $\mathrm{PM} = \sqrt{1999} = 44{\cdot}7102$. The screw itself is cut on a steel tube 8 millimetres in diameter. The tips of the three feet and of the central point are of phosphor-bronze, which does not scratch glass. The arms carrying the three feet are girder-shaped, thin, deep; so as to be very firm vertically; lateral flexure is immaterial. The tips of the three feet are adjusted by filing to chisel-shaped edges, forming little tangents to the imaginary circle of contact. The divided circle is not, as ordinarily, cut upon a flat plate, but is cut upon a thin cylinder of aluminium-bronze affixed to the screw by a light spider with three arms. The whole instrument weighs only 78 grammes. It was constructed for the author by Messrs. Nalder Brothers.

The formula for the focal power of any lens (formula no. (11), p. 207) consists of two factors—one depending solely on the shape of the lens, the other upon its material. The latter factor, $\frac{1-h}{h}$, is a mere numeric; whilst the former, being the difference of two curvatures, is itself a curvature. If the curvature thus determined by shape solely is expressed in dioptries, then, on multiplying by the numeric which depends on the nature of the material, the resultant power of the lens will also be expressed directly in dioptries. The dioptrie spherometer facilitates this desirable end by its directness of reading, quite apart from any incidental advantages which may result from the details of construction.

XXXII. *Researches on the Electrical Resistance of Bismuth.* By Edmond van Aubel, *Doctor of Science**.

A great many memoirs having been published relating to the influence of temperature upon the electrical resistance of bismuth, I think it advisable to sum up in a few lines the actual state of the question, in order to show the bearing of my researches on the solution of the problem.

In the present memoir I have examined the electrical conductivity of bismuth between 0° and 100° only; I will therefore confine myself to stating the results arrived at by physicists between the same limits of temperature.

Matthiessen†, in conjunction with von Bose and other physicists, has found the conductivity of bismuth at 0° to be 1·245, that of silver being taken as 100, and has expressed the conductivity at $t°$ as a function of the conductivity at 0° by the formula

$$\lambda_t = \lambda_0(1 - 0{\cdot}0035216\,t + 0{\cdot}000005728\,t^2).$$

G. Wiedemann, Matthiessen, Holzmann, and Vogt have also studied the effect of the composition of the alloys Bi—Sn, Bi—Pb, upon their electrical conductivity.

According to Matthiessen and Vogt, the electrical conductivity of the alloys of bismuth is modified by the first heating and the first cooling. If λ_0 be the original value, and λ_{01} the conductivity after cooling, we have the following results:—

	λ_0.	λ_{01}.
Pb—Bi (2·27 vol. Pb to 100 vol. Bi) .	8·101	7·633.
„ (18 „ „ „) .	4·558	4·565.

These variations doubtless depend upon permanent changes in the molecular structure.

The conductivity of molten bismuth increases as the temperature is lowered; it diminishes rapidly when the metal solidifies, according to the researches of Matteucci and Matthiessen, and increases again as the solidified mass cools. If small traces of tin or lead be added to the molten bismuth,

* Read June 22, 1889.

† The works of the physicists, whose names will be mentioned later on, are enumerated in the Treatise on Electricity by Prof. G. Wiedemann, vol. i. 1882, p. 503 and the following pages.

according to Matthiessen, the conductivity at first diminishes, as in the case of solid metals, and afterwards increases.

Fr. Weber has found for the specific electrical conductivity of bismuth (C.G.S.) $0{\cdot}838 \times 10^{-5}$; and L. Lorenz has given for the same quantity the values

$$0{\cdot}929 \times 10^{-5} \text{ at } 0^\circ; \text{ and } 0{\cdot}630 \times 10^{-5} \text{ at } 100^\circ.$$

M. Leduc* has observed that the electrical resistance of wires made of an alloy of bismuth and lead increases when the temperature is raised. He has also observed a difference between the electrical properties of wires and of thin plates of commercially pure bismuth, which he attributes to the method of preparation and to the very different rates of cooling.

More recently† the same physicist has found the following results with bismuth which had been run into a tube and then slowly cooled. When the bismuth is heated for the first time, say to 100°, it undergoes an annealing the effect of which is to diminish its initial resistance about 30 per cent. During this operation, between the temperatures 0° and 100°, the following formula is obtained, which must be received with caution:—

$$r_t = r_0(1 + 0{\cdot}00344t + 0{\cdot}0000077t^2),$$

n which r_0 and r_t are the electrical resistances at the temperatures 0° and $t°$ respectively. This formula gives the value $+0{\cdot}00421$ for the mean coefficient of variation of the resistance between 0° and 100°.

Afterwards the resistance varies, between the same limits, according to the formula:—

$$r_t = r_0(1 + 0{\cdot}00375t + 000008t^2),$$

and the mean coefficient of variation of the resistance between 0° and 100° is then 0·00455.

For a thin plate of bismuth, Leduc has found that after annealing the resistance decreases, between 0° and 70°, according to the following formula:—

$$r_t = r_0(1 - 0{\cdot}00158t + 0{\cdot}00000437t^2).$$

* *Journal de Physique*, [2] iii. (1884) p. 362.

† Leduc, Thesis for doctor's degree presented in June 1888 to the Faculty of Sciences of Paris.

And this gives the number −0·00127 for the coefficient of variation with the temperature between 0° and 70°. The resistance of the metal thus prepared would be much greater than that given by Matthiessen, perhaps more than double the value found by him, no doubt because of the difference of molecular structure of the specimens examined.

In 1884, Prof. Righi* published a very remarkable paper, and we will sum up those of his conclusions which bear directly upon our researches.

1. The resistance of commercial bismuth increases on heating between certain limits of temperature, and decreases between others (generally it decreases at temperatures near the ordinary temperature), and, by constructing a curve of resistance, with the temperatures as abscissæ, and the specific resistances as ordinates, we obtain in general a curve in the form of an M. There is a maximum at a low temperature, then a minimum, then a second maximum a little below fusing point, and finally a second minimum after the change of state.

2. The resistance of commercial bismuth varies not only with the temperature, but also with the manner in which the piece has been prepared, and with the temper of the metal.

3. Chemically pure bismuth behaves like other metals; it is not sensibly affected by tempering, and at 0° its resistance compared with that of mercury is 1·15.

4. The difference between pure and commercial bismuth is due to traces of tin, which give to the latter properties similar to those by which steel differs from iron.

5. On adding to bismuth tin in increasing amounts, the specific resistance becomes much greater up to a maximum, and then diminishes.

6. The presence of tin in increasing amounts modifies the curve of resistance, in the same manner as a temper more and more hard would do.

G. Wiedemann† thinks that the phenomena observed by Prof. Righi can be partly accounted for by the discontinuity of the bismuth wires.

* *Journal de Physique*, [2] iii. (1884) p. 355.

† *Elektricität*, Bd. iv., ii., p. 1228.

G. P. Grimaldi* has studied the thermoelectric properties of bismuth, and has confirmed the analogies which Prof. Righi has found between pure bismuth and iron, the bismuth containing tin and steel.

A. von Ettingshausen and W. Nernst† have obtained results which are recorded in the following Table, where x is the electrical conductivity in absolute measure, and α the coefficient of variation of the electrical resistance with the temperature. The plates denoted by Bi were made of absolutely pure bismuth; the other plates denoted by LI to LIV were made of an alloy with tin.

	Bismuth.	Tin.	x (G.S.).	α.
	Parts by weight.	Parts by weight.		
Bi	100		$4{\cdot}80\times10^{-6}$	$-0{\cdot}0012$
LI.	99·05	0·95	$2{\cdot}46\times10^{-6}$	$+0{\cdot}0016$
LII.....	98·54	1·46	$2{\cdot}71\times10^{-6}$	$+0{\cdot}0018$
LIII. ...	93·86	6·14	$3{\cdot}46\times10^{-6}$	$+0{\cdot}0024$
LIV. ...	86·9	13·1	$5{\cdot}62\times10^{-6}$	$+0{\cdot}0025$

The electrical resistance increases then (between 0° and 30°) for every alloy, when the temperature is raised; it decreases on the contrary for pure bismuth under the same conditions. On increasing the quantity of tin, the coefficient of temperature α also increases. The conductivity decreases rapidly by the addition of small quantities of tin, and increases again afterwards.

C. L. Weber‡ has found that the resistance of the metal under consideration at first decreased up to about 100°, the coefficient of temperature being $-0{\cdot}0006$; and then increased up to the melting-point. The position of the minimum, however, is displaced by repeated heatings and coolings between 80° and 120°.

The same physicist has also observed that the electrical resistance of alloys of bismuth and tin, containing from 10

* *Beiblätter zu den Annalen der Physik* (1889), No. 1, p. 25.

† W. Nernst, *Annalen der Physik*, Neue Folge, Bd. xxxi. p. 783 (1887); A. von Ettingshausen and W. Nernst, *Annalen der Physik* (1888), Heft iii. p. 474.

‡ *Annalen der Physik*, 1888, xxxiv. p. 576.

to 80 per cent. of bismuth, increases between 0° and 120°, as the temperature is raised.

In his *Leçons sur l'Eléctricité*, Prof. Exner* mentions that when bismuth is heated the resistance generally decreases, and does not return to its original value on cooling, but reaches a value which is higher as the cooling is more gradual.

Ph. Lenard and J. L. Howard† have studied bismuth wires obtained by means of a screw-press; these wires were rolled into a spiral immediately, while the metal was still hot. They found that in the case of pure bismuth the electrical resistance increased with the temperature between 0° and 36° by 0·0052 for every degree Centigrade.

Finally, in a preliminary communication, published last year‡, I took up again the study of the question. Although bought at the best manufactories in Germany, all the bismuth that I used was very impure.

I examined bismuth slowly cooled, hardened bismuth, and finally compressed bismuth. The molecular structure exerts a great influence upon the electrical properties of different kinds of impure bismuth. All the alloys of bismuth and tin which I studied gave an increase of electrical resistance when the temperature was raised, although the bismuth specimens which entered into the composition of these alloys produced the opposite effect. I also proved, in these preliminary experiments, the great influence of lead as an impurity in bismuth.

If my first results and those of the other physicists be compared with the conclusions arrived at in my present treatise, the wide differences which can be caused by impurities in the pieces of metal under examination will be very striking.

The modes of preparing and of suddenly cooling rods of bismuth have been fully described in a preliminary communication§.

Analysis of different Bismuths.

We have measured the electrical resistances of several

* *Vorlesungen über Elektricität*, Wien (1888), p. 404.

† *Elektrotechn. Zeitschrift*, 1888, Bd. ix., July, Part xiv.

‡ *Bulletins de l'Académie royale de Belgique*, 1888, 3rd Series, xv. No. 1 (Preliminary communication).

§ Phil. Mag. vol. xxv. p. 191; Proc. Phys. Soc. Lond. vol. ix. p. 124.

different bismuths, which we will designate by

Latest Brommsdorff,
Classen I.,
Classen II.,
Classen III.,
Classen IV.,
and pure electrolyzed bismuth.

The first is the metal as pure as it is possible to obtain it commercially. It has been supplied to us by the well-known chemical works of Herr Brommsdorff at Erfurt, who has taken all possible care in its preparation. We have so named this product to distinguish it from Herr Brommsdorff's other bismuths, which we treated of in our "Preliminary Communication."

The qualitative analysis of this metal, made by Prof. Classen, showed that it contains several impurities, principally copper.

The four following bismuths are products as pure as can be obtained by chemical methods of precipitation; they were prepared by Prof. Classen, who used every precaution. The metals Classen I., II., and III. were obtained by reduction from bismuth oxychloride, Classen IV. by reduction from bismuth nitrate.

Bismuth nitrate is a pharmaceutical product which, it appears, can be obtained in a very pure state in commerce.

These different samples were subjected to a minute spectral analysis, the results of which we will point out later, but we may mention here that they all contained lead. Small quantities of lead were always carried down with the precipitated bismuth; this experiment repeated even 13 times never resulted in a pure product. It appears then that *bismuth cannot be obtained absolutely pure by precipitation.*

The last bismuth which we examined, and which we have called "pure electrolyzed bismuth," was prepared by electrolysis. During the electrolysis of a solution of bismuth containing traces of lead, pure bismuth was deposited at the negative pole; and lead, in the form of peroxide of lead, at the positive pole.

As to the lead or tin which were used in the alloys, they

came from Herr Brommsdorff's chemical works, and were sold to me as pure. Besides, they were introduced into the alloys in such small quantities that there was no occasion to take account of any impurities they might have contained.

Purification of Commercial Bismuth.

First Method.

About 250 grams of the metal were dissolved in HNO_3; the solution was brought to the boiling-point in a porcelain dish into which concentrated HCl had been poured. In order to transform all the bismuth nitrate into the chloride, the operation was repeated until the presence of HNO_3 could no longer be detected. The residue was then dissolved in HCl, and alcohol added in successive small quantities. Most of the lead was precipitated in the form of lead chloride, which was removed by filtration. The solution of bismuth was then distributed among 10 glass beakers holding about 4 litres each. The bismuth was precipitated as oxychloride on addition of water. The precipitate was decanted and washed until no trace of HCl could be found. Then it was again dissolved in HCl, the bismuth precipitated as oxychloride, and washed as before. And this was done twelve times. The precipitate finally obtained was carefully washed again, then dissolved in HCl. The bismuth in this solution, to which water had been added, was precipitated by the addition of ammonia and ammonium carbonate. After decantation the precipitate was washed with water, until all the ammonia had disappeared, and dissolved in HCl. This precipitation was repeated three times.

Finally, the precipitate was dissolved in HCl, and the chloride transformed into the oxychloride by addition of water. The resulting precipitate, entirely freed from acid, was dried, mixed with KCN, and reduced. The metallic bismuth was again purified by a second fusion with KCN.

The bismuths Classen I. and Classen III. were both prepared under the direction of Prof. Classen in the laboratory for analytical chemistry at the Polytechnical School at Aix-la-Chapelle.

Classen I. was prepared by Herr Nörrenberg; Classen III.

was obtained by Herr Magdeburg by means of the "Bismuth Purissimum" from the Schucharell works at Görlitz.

Finally, we are indebted to the kindness of Prof. Classen himself for Classen II.

Second Method.

The product used in this second method was the "bismuth subnitrate purissimum" of Dr. Marquardt, at Bonn, which was employed by Marignac in his determination of the atomic weight of bismuth.

One kilogram of this product was dissolved in HCl, and the solution was divided among 22 glass beakers, having a capacity of 4 litres each, and these were filled with water. The precipitate of oxychloride was washed with water until all the HCl was removed. The solution in HCl and the precipitation by means of water were repeated three times. Then the precipitate was again dissolved in HCl, precipitated with ammonia and ammonium carbonate, and thoroughly washed with water. These operations were also repeated three times.

Finally, the precipitate was once more dissolved in HCl, the bismuth oxychloride obtained by the addition of water, and reduced by means of KCN and soda.

Third Method.

The metal, after having been purified by the preceding methods, was subjected to electrolysis to take away completely any trace of lead. Prof. Classen, who is at present engaged in determining the atomic weight of bismuth, intends to describe this electrolytic method in detail.

Researches with the Spectrum.

To produce the bismuth spectrum a large Ruhmkorff's coil with a Leyden jar intercalated was used, and the spark obtained between rods of the metal. The spectrometer employed was one of Meyerstein's with a Schroeder direct-vision prism composed of five separate prisms. The slit was made rather open so as to give a very bright spectrum.

With a smaller dispersion, produced by a fine Merz prism, the spectrum was still more luminous. It was interesting to compare the method of observation which we have just described with that which consists in sending an elec-

tric discharge between platinum electrodes, in a tube containing a solution of a salt of the metal. This method was also used, the rest of the apparatus remaining the same.

It can thus be proved experimentally, and this is of some importance to chemists, *that the first method is much more sure and exact than the other.*

On examining the bismuth "Latest Brommsdorff" in the spectroscope, sodium and copper were found to be present. The two characteristic lines D_1 D_2 of sodium remained, even when the surface of the bismuth rods had been well cleaned by long immersion in nitric acid. The metal contained only traces of lead.

The other bismuths contained no copper and no sodium. The *traces* of lead, which were found in the bismuths Classen I., II., III., IV., and in the "Latest Brommsdorff," were all very faint; indeed we were not able to establish them with certainty, except by the following process :—

After having substituted, for the original rods of bismuth, other rods of lead, so as to produce a good spectrum of this latter metal, the point of cross section of the micrometer wires of the spectroscope was placed upon the most visible line of the lead spectrum, and then the rods of lead were replaced by those of bismuth.

The bismuth Classen I. showed the very characteristic line of lead, of wave-length 5610·4.

The same line was found in Classen II. and Latest Brommsdorff; but it was much more feeble. Classen III. and Classen IV. also contained lead.

To sum up, all these bismuths contained lead in variable amounts, Classen II. and Latest Brommsdorff containing the least.

But in the case of the bismuth obtained by electrolysis, no impurity was discovered by spectrum analysis.

This very minute spectrum analysis of products, prepared with the greatest care by such a distinguished chemist as Prof. Classen, warrants us in saying that *pure bismuth cannot be obtained with certainty by precipitation. The electrolyzed metal can alone be considered as chemically pure.*

In the successive precipitations the traces of lead are drawn down mechanically.

In electrolyzing the solutions of bismuth, in which the spectrum analysis had revealed traces of lead, a very thin but perfectly visible coating of peroxide of lead was found at the positive pole.

Calculation of the Absolute Values of the Electrical Resistances.

The mode of measuring the electrical resistance has been described in the preliminary communication.

In order to determine the absolute value of the electrical resistances at 0° of the tempered rods, it was necessary to calculate the mean section of these rods from their weight, density, and length.

No sensible error is committed in taking 9·82 as the density of bismuth or of the alloys which we have studied, as the lead and tin contained in the latter were only present in very small quantities. Besides, a much greater cause of error is the exact determination of the length of the rods of bismuth between the two solderings.

In the case of the slowly cooled rods the difficulty is greater, because the bismuth adheres very closely to the glass tubes in which it is contained *, and because these tubes cannot be considered as cylindrical. In spite of every precaution it is impossible to break the glass, so that the whole of the metal may be got out clear without breaking the rod of bismuth. So I measured with a spherometer the diameters of the bismuth rod at the two extremities and in the middle ; and I considered the rod as formed of two truncated cones joined together at the small end. It was then easy to calculate the electrical conductivity at 0°; for example, either using the formula given by Siemens (*Annalen der Physik*, vol. cx. 1860, p. 3, or F. Kohlrausch, *Guide de Physique pratique*, édition française, p. 223), or calculating the section of a cylinder whose height is the length of the rod of bismuth, and whose volume is that of the two truncated cones joined together. The section of this cylinder can then be taken as the mean section of the rod.

All the absolute values that I give further on are thus only approximations, but they approach very near to the true values.

* Righi, *Journal de Physique*, 2nd series, vol. iii. 1884, p. 132.

Results of the Electrical Measurements.

The following Tables contain, in column W, the electrical resistances as I have measured them, that is to say in Siemens units, and at different temperatures given by a Centigrade thermometer. I have given also, for each bismuth, the specific electrical resistance R_0 in C.G.S. units at 0° temperature; and the mean coefficient of variation of the electrical resistance between 0° and 100°, *i. e.* the quantity K of the equation $R_t = R_0(1 + Kt)$.

Bismuths.

(1) Rods which have been slowly cooled.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Latest Bromms-dorff	0°	0·1390		
	19	0·1462		
	49·1	0·1590	+0·00325	$10^3 \times 109{\cdot}90$
	99·6	0·1840		
Classen I.	0	0·2875		
	19·1	0·2882	+0·00076	
	49·2	0·2922		
	99·6	0·3095		
Classen II.....	0	0·1615		
	16·7	0·1680	+0·00299	$10^3 \times 124{\cdot}69$
	49·6	0·1828		
	99·5	0·2096		
(2) Classen III. 2nd rod.	0	0·2447		
	20·5	0·2500	+0·00161	
	49·8	0·2595		
	99·8	0·2841		
(1) Classen III. 1st rod.	0	0·2407		
	20·5	0·2439	+0·00132	$10^3 \times 156{\cdot}74$
	49·7	0·2509		
	99·8	0·2725		
Classen IV. 1st rod.	0	0·2036		
	20·3	0·2063		
	99·8	0·2302		
	It is allowed to cool slowly.			
	0	0·2043		
	19·9	0·2066		
	It is heated to 100°, and allowed to cool slowly.			
	0	0·2048		
	20	0·2069	+0·00126	$10^3 \times 170{\cdot}07$
	52·5	0·2136		
	99·6	0·2305		
	It is allowed to cool again slowly, and it is found that the values remain constant.			

Table (*continued*).

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen IV. 2nd rod.	0	0·1790		
	20·2	0·1806		
	99·8	0·1998		
	It is allowed to cool slowly.			
	0	0·1794		
	20	0·1810		
	It is heated to 100°, and allowed to cool very slowly.			
	0	0·1798		
	20	0·1810	+0·00113	$10^3 \times 168{\cdot}35$
	52	0·1861		
	99·6	0·2000		
	It is allowed to cool again slowly, and it is found that the values remain constant.			
Pure electrolyzed Classen Bismuth, 1st rod.	0	0·1031		
	99·61	0·1490	+0·00447	
	It is allowed to cool very slowly.			
	0	0·1032		
	99·65	0·1491		
	It is allowed to cool again very slowly.			
	0	0·1032	from 0° to 19°·5 : +0·00412	
	19·5	0·1115	from 0° to 55° : +0·00426	
	55	0·1274	from 0° to 99°·72: +0·00447	
	99·72	0·1493		
Pure electrolyzed Classen Bismuth, 2nd rod*.	0	0·0912		
	99·6	0·1322		
	It is allowed to cool very slowly.			
	0	0·0913	from 0° to 22°·1 : +0·00411	
	22·1	0·0996	from 0° to 56° : +0·00426	$10^3 \times 107{\cdot}99$
	56	0·1131	from 0° to 99°·7 : +0·00450	
	99·7	0·1323		

* We think it will be interesting to give, for the pure electrolyzed bismuth, the values of the influence of magnetism on the electrical resistance.

ΔW being the difference between the electrical resistance in the magnetic field and outside of it.

Temperatures.	$100\frac{\Delta W}{W}$.	Intensity of magnetic field.
0	2·9	about 1560 C.G.S. units.
99·7	0·415	

(2) Tempered Rods.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Latest Brommsdorff.	0°	0·0965		
	17·6	0·0988	+0·00199	$10^3 \times 139{\cdot}86$
	49·3	0·1040		
	99·7	0·1157		
Classen I. ...	0	0·1745		
	17·5	0·1729		
	49·1	0·1687	−0·000603	$10^3 \times 246{\cdot}91$
	99·7	0·1640		
Classen II. 1st rod.	0	0·1275		
	16·7	0·1287		
	49·7	0·1319	+0·00106	$10^3 \times 166{\cdot}66$
	99·5	0·1410		
Classen II. 2nd rod.	0	0·1292		
	16·9	0·1310		
	50·3	0·1353	+0·00128	$10^3 \times 157{\cdot}48$
	99	0·1456		
Classen II. 3rd rod.	0	0·1205		
	18	0·1215		
	99·8	0·1342		
	It is allowed to cool slowly.			
	0	0·1203		
	99·7	0·1343	+0·00116	$10^3 \times 163{\cdot}40$
	A second observation showed that the resistances now remain constant.			
Classen III. 1st rod.	0	0·1135		
	19·7	0·1127		
	99·8	0·1139		
	It is allowed to cool slowly.			
	0	0·1128		
	20	0·1121	+0·00009	$10^3 \times 204{\cdot}50$
	48	0·1114		
	99·8	0·1138		
	A second set of observations of the electrical resistances from 0° to 100° showed that the values remained constant.			
Classen III. 2nd rod.	0	0·1376		
	20	0·1363		
	99·8	0·1372		
	It is allowed to cool slowly.			
	0	0·1364		
	20	0·1355	+0·00005	$10^3 \times 208{\cdot}33$
	48	0·1346		
	99·8	0·1371		
	A second set of observations showed that the values of the resistances remained constant.			

Table (*continued*).

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen IV. 1st rod.	16·6	0·1301		
	49·6	0·1285		
	99·7	0·1301		
	It is allowed to cool rather slowly.			
	0	0·1307		
	16·6	0·1295		
	49·3	0·1281	−0·00004	$10^3 \times 207{\cdot}47$
	99·7	0·1302		
	A second investigation showed that the values of the resistances remained constant.			
Classen IV. 2nd rod.	16·5	0·1507		
	49·6	0·1486		
	99·7	0·1501		
	It is allowed to cool rather slowly.			
	0	0·1512		
	16·5	0·1500		
	49·5	0·1479	−0·00008	$10^3 \times 212{\cdot}94$
	99·7	0·1500		
	A second investigation showed that the values of the resistances remained constant.			
Pure electrolyzed Classen Bismuth. 1st rod.	0	0·0965	+0·00434	
	99·6	0·1382		
Pure electrolyzed Classen Bismuth. 2nd rod*.	0	0·0952		
	99·6	0·1370		
	It is allowed to cool very slowly.			
	0	0·0950	from 0° to 21°·9 : +0·00399	
	21·9	0·1033	from 0° to 56°·1 : +0·00422	
	56·1	0·1175	from 9° to 99°·7 : +0·00445	$10^3 \times 108{\cdot}69$
	99·7	0·1372		

* We think it will be interesting to give the value of the magnetic action for this rod of hardened bismuth, as we have done elsewhere.

Temperatures.	$100\frac{\Delta W}{W}$.	Intensity of magnetic field.
0	2·89	about 1560 C.G.S. units.
99·7	0·402	

(3) Compressed Rods.

Bismuths.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
Classen II. 1st rod.	20°·6	0·1300		
	It was observed that the electrical resistance was permanently changed each time the rod of bismuth was heated. Finally, after having been heated from 0° to 100°, and been allowed to cool from 100° to 0° several times, constant values were found. We cannot tell what is the cause of this considerable decrease in the resistance, but it is certainly not due to an alteration of the connexions.			
	0	0·1250		
	22	0·1215	−0·00049	$10^3 \times 236{\cdot}96$
	99·4	0·1189		
Classen II. 2nd rod.	20·5	0·1353		
	Same observation as above.			
	0	0·1352		
	21·9	0·1302	−0·00083	$10^3 \times 251{\cdot}26$
	99·3	0·1241		
Classen II. 3rd rod.	0	0·1019		
	18	0·0978		
	99·8	0·0913		
	It is allowed to cool slowly.			
	0	0·1024		
	99·7	0·0916	−0·00105	$10^3 \times 268{\cdot}10$
	A further investigation showed that the resistances then remained constant.			

Alloys with Tin.

Alloys.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
1st. Rods slowly cooled.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0°	0·2670		
	19	0·2710		$10^3 \times 458{\cdot}71$
	49	0·2715		
	99·6	0·2623		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·6790		
	16·7	0·6917		$10^3 \times 416{\cdot}66$
	49·6	0·6890		
	99·5	0·6540		
2nd. Tempered Rods.				
Latest Brommsdorff bismuth + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1960		
	17·7	0·2020		$10^3 \times 346{\cdot}02$
	49·3	0·2083		
	99·7	0·2095		
Bi Classen II. + Sn. 0·5 gr. Sn to 100 gr. Bi.	0	0·1795		
	17·5	0·1844		$10^3 \times 354{\cdot}61$
	49·2	0·1905		
	99·7	0·1915		
Bi Classen II. + Sn. 0·53 gr. Sn to 100 gr. Bi.	0	0·2561		
	16·7	0·2637		$10^3 \times 349{\cdot}65$
	49·9	0·2735		
	99·5	0·2780		

Alloys with Lead.

Alloys.	Temperatures.	W. Electrical resistances in U.S.	K. Variation for 1° between 0° and 100°.	R_0.
1st. Rods slowly cooled.				
Latest Brommsdorff Bi + Pb. 0·5 gr. Pb to 100 gr. Bi.	0	0·2125		
	19·1	0·2122		
	69·0	0·2127		
	99·6	0·2200		
Bi Classen I. + Pb. 0·5 Pb to 100 gr. Bi.	0	0·4037		
	19	0·3887		$10^3 \times 362{\cdot}32$
	49·1	0·3690		
	99·6	0·3530		
2nd. Tempered Rods.				
Latest Brommsdorff Bi + Pb. 0·5 gr. Pb to 100 gr. Bi.	0	0·1420		
	17·6	0·1425		
	49·2	0·1400		$10^3 \times 245{\cdot}70$
	99·7	0·1365		
Bi Classen I. + Pb. 0 5 gr. Pb to 100 gr. Bi.	0	0·1475		
	17·5	0·1485		
	49·1	0·1470		$10^3 \times 274{\cdot}27$
	99·7	0·1414		

Conclusions.

I have observed that the electrical resistances of some rods were changed permanently after the first heating; but when once the values remain constant they do not change again, even after several months. Dr. Leduc* has observed a similar phenomenon. Rods of the same bismuths, either slowly cooled or hardened under the same conditions, give nearly the same values for the resistance at 0° and for the coefficient of variation with the temperature. The methods of tempering and of slowly cooling remain sensibly the same for the different specimens. We must not then look to this cause to explain the great variations which have been observed between one bismuth and another.

The molecular structure, which I have changed by tempering and by compression, makes a great difference in the

* Thesis for doctor's degree, p. 30.

electrical properties of impure bismuths. On the other hand, tempering appears to have no action whatever on pure bismuth ; thus, the electrical resistance at 0° is :—

for electrolyzed bismuth, slowly cooled . . $10^3 \times 107{\cdot}99$,
and for the same metal, tempered . . . $10^3 \times 108{\cdot}69$.

The coefficient of variation with the temperature and the influence of magnetism are very nearly the same for these two specimens. The coefficient of variation with the temperature besides is *positive*.

In the case of impure bismuths the process of tempering causes the coefficient K to decrease, and even to become negative. The action of compression seems to be still greater.

As to the absolute values of the electrical resistance at 0°, they increase under the action of tempering and compression.

If we compare the results furnished by the electrolyzed bismuth with those given by the other bismuths, we see that the effect of traces of lead is to produce a diminution in the value of K, and an increase in the value of R_0. It can also be seen that, in the case of impure metals, a high value for R_0 generally corresponds to a low value for K.

With regard to alloys of impure bismuth with lead and tin, the results prove that these latter metals tend to increase R_0, and can even in certain cases give a negative value for K. But the molecular action, and above all the lead, produce a much greater effect than the tin.

The electrolyzed bismuth presents a peculiarity which is not seen in the impure kinds. The coefficient K remains sensibly the same at different temperatures between 0° and 100°, and this may be considered as a proof of the purity of the metal.

In short, one may say that, of all the methods both physical and chemical, the determination of the electrical resistance is certainly the most exact for ascertaining if the bismuth be pure, and above all if it contain no trace of lead. While spectrum analysis and the process of electro-

lysis have with difficulty discovered the existence of lead in the bismuth Classen II. for example, the study of the electrical resistance leads to very different results.

When one considers all the difficulties I have met with in procuring the pure metal, and the number of electrical measurements which I have been obliged to make, so to speak, uselessly, it is plain that, before studying the physical properties of a metal, it would always be wise to submit the metal to a very careful spectrum analysis, to assure one's self of its purity.

The differences, which I pointed out at the beginning, between the results of other physicists are easily explained by the rarity of pure bismuth.

Unfortunately I had not enough of pure bismuth to be able to study compressed electrolyzed bismuth and its alloys with tin and lead.

Considering the great influence of very small traces of lead, it is allowable to suppose that the presence of this foreign metal modifies the molecular structure of the bismuth to a considerable extent; unfortunately a microscopic examination could hardly give any result here.

Finally, if the results which we have obtained in this treatise be compared with those which we published in our "Preliminary Communication," it will be seen how much the conclusions deduced from the experiments may vary according to the nature of the impurities contained in metal. Also, the question of the variation of the electrical conductivity of bismuth with the temperature presents a *difficulty of a chemical nature*, and we strongly recommend physicists to make known in their works the method of preparation, and if possible the results of the analysis of the different bismuths which they have examined.

By means of the two following Tables my results may be easily compared with those of other physicists. It appears by the results that the bismuths studied by Messrs. L. Lorenz and Righi were pure.

Bismuths.	Slowly cooled.		Tempered.		Compressed.	
	R_0.	K *.	R_0.	K.	R_0.	K.
ommsdorff...	$10^3 \times 109{\cdot}90$	0·00325	$10^3 \times 139{\cdot}86$	0·00199		
assen I.	.....	0·00076	$10^3 \times 246\,91$	−0·000603		
assen II. *a* ...	$10^3 \times 124{\cdot}69$	0·00299	$10^3 \times 166{\cdot}66$	+0·00106	$10^3 \times 236{\cdot}96$	−0·00049
II. *b* ...	.....	.....	$10^3 \times 157{\cdot}48$	+0·00128	$10^3 \times 251{\cdot}26$	−0·00083
II. *c* ...	.....	.....	$10^3 \times 163{\cdot}40$	+0·00116	$10^3 \times 268{\cdot}10$	−0·00105
assen III. *a*...	$10^3 \times 156{\cdot}74$	0·00132	$10^3 \times 204{\cdot}50$	+0·00009		
III. *b*...	.....	0·00101	$10^3 \times 208{\cdot}33$	+0·00005		
assen IV. *a*...	$10^3 \times 170{\cdot}07$	0·00126	$10^3 \times 207{\cdot}47$	−0·00004		
IV. *b*...	$10^3 \times 168{\cdot}35$	0·00113	$10^3 \times 212{\cdot}94$	−0·00008		
ectrolyzed Bi.	$10^3 \times 107{\cdot}99$	0·00429	$10^3 \times 108{\cdot}69$	+0·00422		

* It is to be observed that all the coefficients of K in this column are *positive*.

Observations.	Experimenter.	R_0.	K.
	Lenz.	$10^3 \times$ 82·36	
	Matthiessen.	$10^3 \times 131{\cdot}50$	
	Matthiessen.	$10^3 \times 125{\cdot}70$	+0·00418
	F. Weber.	$10^3 \times 119{\cdot}33$	
	L. Lorenz.	$10^3 \times 107{\cdot}64$	+0·00475
Bi cooled slowly.	Leduc.		+0·00455
Bismuth plate.	Leduc.		−0·00127
	Righi.	$10^3 \times 108{\cdot}49$	
Bismuth plate.	Von Ettingsh. & Nernst.	$10^3 \times 208{\cdot}33$	−0·0012
	C. L. Weber.		−0·0006
	Lenard & Howard.		+0·0052
Bi cooled slowly.	Edw. van Aubel.	$10^3 \times 107{\cdot}99$	+0·00429
Bi hardened.	Edw. van Aubel.	$10^3 \times 108{\cdot}69$	+0·00422

I have great pleasure in expressing my thanks to Profs. Wüllner and Classen for having so kindly furnished me with the valuable materials necessary for my researches.

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